The Causes of Variation in Human Cooperative Behaviour

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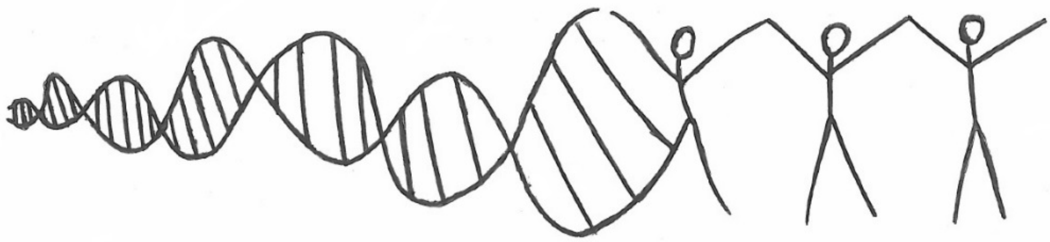

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Abstract

This thesis investigates variation in human cooperative behaviour in naturally occurring contexts. I critically assess the prevailing consensus on human cooperation derived from laboratory games (such as the dictator and public goods games), by identifying real life analogues and conducting extensive field observation and experiments. My second chapter investigates the importance of context on social behaviour by taking a commonly used laboratory game, the dictator game, and studying analogous behaviour, giving to mendicants in the street. I conclude that individuals cooperate less in the wild than they do in the laboratory and that monetary pay-offs are important in cooperative decision-making. My third chapter examines how social cues influence peoples' likelihood of giving to mendicants. I conclude that increased group size and crowd density negatively affect donation behaviour. My fourth chapter investigates dog fouling in public parks to understand the causes of variation in cheating in a naturally occurring public goods game. I conclude that despite evidence that a social game is being played, the cues that influences decisions are unclear, and behaviour may depend on local social norms. My fifth chapter investigates social influences on red light jumping by cyclists at pedestrian crossings. I find that the probability of cheating is higher with fewer observers and when other cyclists also cheat.

Lay Summary

This thesis addresses the topics around what drives people to cooperate, or help, one another. This is not a simple problem. Evolutionary theory expects people to behave selfishly. Cooperation poses a problem because at the moment that someone decides to cooperate with another person they may not know whether that help will be repaid. This topic has often been investigated by recruiting participants to play simple games in laboratories. This approach is problematic for several reasons: (1) People are intensely aware of when they are under observation and change their behaviour; (2) People change their behaviour when they know they are in an experiment; (3) It doesn't cost anything to take part in an experiment, so the decisions participants make aren't "real"; (4) There is a bias towards students taking part in these experiments instead of the general population.

My second chapter addresses this by taking one of games that is commonly used in the laboratory, the dictator game, and running experiments in a naturally occurring analogue of this game: pedestrians giving to individuals begging in the street. I found that people give less frequently in this context. I also found that they give more frequently if they have just found some money and most frequently if you stop people in the street and invite them to play the dictator game as you would in the laboratory.

My third chapter extends this study by asking what other social factors influence how people make decisions about giving in the street. I conclude that pedestrians' group size and crowd density are important. With people giving most frequently when they are on their own, and when the density of pedestrians is lower.

My fourth chapter looks at a different cooperative game, the public goods game. In these games, individuals must pay a cost to participate but all participants gain the benefit of this participation. The real life situation I used was dog poo in public parks, where individual dog walkers pay the cost of picking up poo but all park users gain the benefit of this behaviour. I conclude that whilst it seems that there is public goods game being played in parks, the cues that people use to decide whether to pick up aren't clear and might depend on long standing local habits.

My fifth chapter looks at a different public goods game, cyclists jumping red lights. I found that cyclists were influenced by the density of pedestrians around them and jumped more frequently when there were fewer pedestrians. Cyclists also influenced each other's behaviour and were more likely to jump the lights when they saw others doing so.

Declaration

The work in this thesis has been carried out by myself with guidance from my supervisors, unless otherwise stated and detailed below. This thesis is of my own composition and has not been submitted for any other degree or professional qualification.

Chapter 2 Amy Munro-Faure carried out data collection with assistance from Dagmar Der Weduwen, Iris Berger and Rachel Murray-Watson. Stuart Sharp and Henry Ferguson-Gow offered additional comments on the text.

Chapter 3 AMF led data collection and was assisted by DDW, IB, RMW, Emma Hunter, Ellen Bird, Ben Murphy and Will Johnston. Liam Brierley offered advice on statistical analysis.

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All studies were reviewed and approved by the School of Biological Science's ethical reivew committee.

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Contents

Chapter 1 - Introduction

1.1 Thesis Rationale

The success of humans, as a species, depends in large part to our ability to cooperate with one another in a complex way (Dunbar, 2003). Humans are unusual in that society is characterised by cooperation between non-relatives on a large scale (Melis and Semmann, 2010). There is, however, substantial variation in the intensity and stability of this cooperation, for example, people regularly break the law (Harcourt, 1998). The causes of this variation are not fully understood (Holland et al. 2012; Burton-Chellew & West 2013; West et al. 2007). Studying human cooperation is therefore interesting both from an academic point of view but also from a practical one. Understanding the mechanisms that stabilise cooperation could help resolve situations where cooperation breaks down.

1.1.1 Why study cooperation?

Cooperative behaviour can be found at all biological levels (West et al. 2007). For example, the interaction of slime moulds with one another to produce fruiting bodies, the interaction of genes on the genome to promote each others propagation, the look-out behaviour of meerkats for predators and food sharing amongst primates (Jaeggi and Van Schaik, 2011; Santema and Clutton-Brock, 2013; Santorelli *et al.*, 2013; Penny, 2015). In a behavioural ecological framework cooperation can be defined as any interaction that provides a net benefit to all interactants as opposed to altruism where one individual pays a net cost to benefit others (Trivers 1971; Fehr & Fischbacher 2003). Cooperative interactions can be divided into two main classes when we consider the benefits and costs at the moment of interaction: mutualism and conditional cooperation. In a mutualism all interactants have an immediate positive pay-off and there is no opportunity or incentive for individuals to cheat. In conditional cooperation interactants have an opportunity to cheat, for example, there may be a time delay between each interactants cooperative action In conditional cooperation even if both interactants' ultimately gain a net benefit, the opportunity for cheating can allow a cooperative interaction to turn into a selfish one (Dufwenberg and Kirchsteiger, 2004). The mechanisms that stabilise conditional cooperation allow individuals to monitor each others behaviour and maintain net pay-offs such that they are positive for all interactants (West et al. 2007).

The mechanisms that stabilise conditional cooperation include kin selection, when individuals are related to one another they can gain indirect genetic benefits by cooperating with one another (Clutton-Brock, 2002; Rumbaugh *et al.*, 2012). Between non-relatives the key mechanisms for stabilising conditional cooperation are reciprocity, you scratch my back and I'll scratch yours, and punishment of uncooperative behaviour.

The terms cooperation and altruism are widely used by different fields and lay people. There is a swathe of different meanings for both words depending on what discipline you look to. For example, within social psychology the term cooperation is used to describe the majority of "helping" behaviours regardless of whether these fit into the slightly more constrictive definition outlined above (Levine *et al.*, 1994). Similarly altruistic behaviour is often regarded by sociologists as any behaviour where an individual imposes any type of cost on themselves in order to help regardless of the net outcome, much as I have described conditional cooperation above. In this thesis I will use the terms altruism and cooperation to describe the net costs and benefits to an organism. An altruistic behaviour being one where an organism ends with a net cost by helping and a cooperative one being when an organism ends with a net benefit, even if they impose costs on themselves to ultimately gain this benefit.

The theoretical construct of the public goods game underlies a huge proportion of cooperative behaviour. A public goods game describes a situation where individuals contribute to a common cause which benefits all group members, providing an incentive to cheat. Cooperation disintegrates rapidly when repeated public goods games are played between non-relatives in the laboratory, except where individuals can punish one another, or their reputation is at stake (Fehr and Gachter, 2000).

1.2 Why people?

1.2.1 Extent and sophistication of human cooperative behaviour.

People are a unique example of extreme cooperative behaviour (Melis and Semmann, 2010). However, people are characterised by their sociality (Dunbar, 2003). Whilst the reasons for the evolution of this sociality are the same that drive sociality in other species, the sociality that people exhibit is ubiquitous and complex (McNally, Brown and Jackson, 2012). Many authors have argued that people owe their evolutionary success and large population size to their ability to work together (Boyd & Richerson, 2009). The development of language has been cited as a facilitator of complex social interactions and as method by which people can, share information about others reputations through gossip and assess reliability of potential co-operators (Dunbar, 2003).

People are also interesting for their propensity to engage in both one shot and repeated cooperation with non-relatives (Rachlin and Jones, 2008; Raihani and Bshary, 2015). One-shot cooperative behaviour with non-relatives can present a dilemma, these interactions are very common, individuals may not both immediately receive a pay-off and yet human society is largely stable (Boyd *et al.*, 2003). One off cooperative interactions include many transactional interactions such as paying a bus fare. The reasons that people do cooperate in these instances tend to be because they are enshrined in law or local social norms such that people know that they will be punished if they do not cooperate or may lose out on future cooperative interactions in the instance of social norms (Dufwenberg and Kirchsteiger, 2004). Despite these structures there is variation in human cooperative behaviour. Individuals do break the law and do not always abide by local social norms (Fehr and Fischbacher, 2004; Johnson *et al.*, 2011). Asking what causes this variation in people's cooperative decision-making is therefore an interesting avenue for research.

1.2.2 What forms human cooperation takes.

We describe cooperation using several theoretical frameworks. One of the most common is the public goods game. In single shot public goods games, the proportion of individuals choosing to contribute is unexpectedly high. Both evolutionary and economic explanations of human behaviour suggest that rational selfish actors should not contribute in such games (Levitt and List, 2008). The principal mechanism that explains high cooperation in both single shot games and the early rounds of iterated games is that individuals cooperate conditionally and adjust their strategy over time (Fischbacher, Gächter and Fehr, 2001; Kocher *et al.*, 2008). So, although cooperation in a single shot games appears irrational, subjects are learning how to play and strategise more rationally. Most empirical observations of public goods games do not produce results that are consistent with either economically optimal behaviour (invest all your endowment), or the Nash equilibrium of invest nothing (Isaac and Walker, 1988; Fischbacher, Gächter and Fehr, 2001). This game theoretic optimum of defection may only be a solution for public goods dilemmas that reward defection, including the prisoners' dilemma. This may explain some of the variation in investment in public goods dilemmas by experimental subjects, however, there is currently little agreement as to what other alternative explanations are important.

The theoretical structure of a public good game underlies many real life interactions but research on how people cooperate in real life situations is minimal (Levitt *et al.*, 2007). One study showed a correlation between how much people contributed to a laboratory game and the gauge of fishing net they used, suggesting that there is a cooperative personality (Fehr and Leibbrandt, 2011). Other studies have not shown these within individual effects and suggest that context is more important in decision-making (Thøgersen, 2008). There can be an interaction between public and private goods. If individuals have a long term stake in or ownership of part of a public good, this can motivate them to address more heavily in it and resolve the dilemma (Hardin, 1968). There is a paucity of studies that address what cues individuals use to make decisions in real life public goods dilemmas.

There are several other economic games that behavioural economists use in the laboratory as analogues for cooperative behaviour. The simplest of these is the dictator game (Levitt and List, 2007). In this game one experimental subject is given an endowment by the experimenters. The subject is then given the opportunity to split this endowment with another anonymous unrelated individual. This game was initially devised as a three person game to test fairness norms but has gone on to be used extensively as a measure of prosocial behaviour (Kahneman, Knetsch and Thaler, 1986; Engel, 2011). The general finding from laboratory studies of dictator games is that people are more prosocial than theory would predict (Levitt *et al.*, 2007; Engel, 2011). About half of subjects choose to split their endowment (Engel, 2011). There are several other factors that have been shown to affect cooperation in dictator games including reputational effects and the size of pay-off that subjects receive (Haley and Fessler, 2005; Dana, Cain and Dawes, 2006; Brosig-Koch, Riechmann and Weimann, 2017).

Other games that have been used to study cooperative behaviour in humans include the ultimatum game, trust game, snowdrift and staghunt game (Levitt *et al.*, 2007; Melis and Semmann, 2010). Whilst results from these games all continue the theme of unexpected prosocial behaviour in the laboratory, I will not discuss them further here.

1.2.3 How human cooperation is maintained.

The mechanisms that maintain cooperation can be divided into two main groupings: Mutualisms and Conditional Cooperation.

1.2.3.1 Mutualisms

A mutualism describes a situation where all interacting parties obtain a benefit. There is no dilemma associated with mutualisms as all parties that engage in the interaction obtain an immediate net gain (West, Griffin, & Gardner, 2007). Examples of mutualisms include coordinated hunting by different species. For example, groupers and giant Moray eels hunt together in the Red Sea and obtain higher yields than they would do on their own (Bshary *et* *al.*, 2006). In this instance the co-operators are not related to one another but both gain an instant benefit of increased hunting success by working together. In human populations mutualistic interactions would describe any similar interaction, for example, the theoretical stag hunt game describes where to catch a valuable prey item you need a certain number of hunters (Fang *et al.*, 2002; Duguid *et al.*, 2014). If hunters do not co-ordinate they can only acquire lower value prey items, this type of interaction has been observed in natural systems, for example in subsistence sperm-whale hunting in Indonesia (Alvard and Nolin, 2002). A more pertinent example for modern humans is social prestige. If a wealthy benefactor publicly donates to a charitable cause then the charity receives a financial benefit whilst the benefactor gains a charitable reputation (Sylwester and Roberts, 2010; Bonini, Court and Marchi, 2014). Whilst mutualisms occur in human society they pose less of a theoretical problem to be explained than conditional cooperation.

1.2.3.2 Conditional Cooperation: Reciprocity and Reputation

Reciprocity describes "you scratch my back, I'll scratch yours" interactions, where one individual does something to benefit another and the recipient of that action repays this (R. L. Trivers, 1971). In reciprocal interactions there is a time delay between the two actions which open them up to the possibility of cheating (Ghoul *et al.*, 2014). For reciprocity to be stable individuals must cooperate conditionally with individuals that they believe will repay their initial investment but not with those who cheat (Stuart A West, Griffin and Gardner, 2007). In order for individuals to acquire reliable information as to whether or not their cooperating partner is reliable, reputational information is important. To retain a good cooperative reputation we might expect individuals to engage in activities that maintain this reputation. For example, people adjust their behaviour to be more cooperative when they are aware that they are under observation (Yoeli *et al.*, 2013). People will also behave more cooperatively when they receive gentle proxies of observation like posters with eyes on them (Haley and Fessler, 2005; Bateson, Nettle and Roberts, 2006).

Cooperation can be mediated by immediate direct interactions between individuals but also through indirect interactions via third parties. Indirect reciprocity describes where an individual performs a costly behaviour for a recipient who is part of a group, one of whom ultimately performs a costly behaviour for the original actor (Nowak and Sigmund, 1998). To avoid being cheated in indirect interactions people use reputations to assess likelihood of defection in these cooperative decisions (Boone & Buck, 2003; Gintis & Fehr, 2012;). The importance of indirect reciprocity in human cooperation is demonstrated by the behavioural shift towards elevated cooperation when people are aware they are being observed (Bateson, Nettle and Roberts, 2006; Yoeli *et al.*, 2013).

Indirect reciprocity can be mediated through group membership so individuals cooperate more with individuals in their group in the expectation that they will be repaid (Nowak, 2006). The existence of inter-group competition can stabilise in-group cooperation and groups with higher in-group cooperation may outcompete other groups (Puurtinen and Mappes, 2009). There is evidence that there is a limit to the number of individuals that people can recall reputational information about and that this number correlates with tribe size in hunter-gatherer societies (Dunbar, 1993; Mac Carron, Kaski and Dunbar, 2016). In-group interaction may be an important mechanism by which individuals cooperate conditionally. One facet of group behaviour can be local social norms, where individuals in the same location behave similarly (Ostrom, 2000; Fowler and Christakis, 2010). This type of in-group behaviour may be the basis of social contagion where individuals copy behaviour from others that they see (Christakis and Fowler, 2010; Lacetera, Macis and Mele, 2015). The conditional nature of most cooperation may explain why social contagion is not universal (Tsvetkova and Macy, 2014).

1.2.3.3 Conditional Cooperation: Punishment

Punishment goes further than reciprocity as cheats lose out not only on future interactions but also experience an immediate direct cost (Boyd et al., 2003; Clutton-Brock & Parker, 1995). Punishment is a universal feature of human society (Henrich *et al.*, 2006). In economic laboratory games the possibility of punishment maintains cooperation in both one-shot games and over multiple iterations (Fehr and Gächter, 2002; Gächter, Renner and Sefton, 2008). In these games cooperating individuals are able to immediately impose a cost on defectors. Cooperation is only maintained in treatments where participants are allowed to punish (Fehr and Gächter, 2002). Punishing defection appears to be more effective than rewarding cooperative behaviour (Gächter, 2012). In most laboratory games punishment is also costly to the punisher so is often termed altruistic punishment (Boyd *et al.*, 2003). Punishment behaviour can be viewed as coercion where punished individuals act to minimise their costs (El Mouden, West and Gardner, 2010; Raihani, Thornton and Bshary, 2012). This could explain why cooperation is seen in one-shot interactions between unrelated individuals, as individuals act cooperatively to avoid punishment.

1.3 What are the problems with the status quo?

1.3.1 Conceptual Problems

Human cooperative behaviour appears to be context-dependent and the majority of current research, whilst often acknowledging this, fails to address it (Slovic, 1995; Hoeffler and Ariely, 1999). The body of work initiated by modern behavioural economists, whilst giving us insight into relatively subtle determinants of our behaviour, frequently fails to acknowledge that pay-offs associated with behaviour will often strongly drive decision-making (Burton-Chellew and West, 2013). Evolutionary and economic theory both suggest that we would expect individuals to change their behaviour when the associated pay-offs change (Gintis, 2000; Hamilton, 1964). This problem of context dependence cuts in two ways: 1) It makes it impossible to generalise from lab experiments; 2) It may drive a great deal of currently unexplained variation in cooperative behaviour.

A second problem is that the majority of laboratory models are unrealistic. We have little quantitative evidence of whether or not the types of structures studied by behavioural economists relate to how people behave in real life. Structures like the dictator game, whilst useful models, seem very unlike the majority of real life interactions. Indeed this lack of similarity might explain the lack of rigorous studies on these types of games in naturally occurring situations. In short, we have limited quantitative evidence that these structures are actually how people behave in real life.

Finally, there is a tension amongst human behavioural scientists about the most appropriate way to collect and analyse data. Within psychology there is a phenomenon that has become known as the replication crisis, where researchers have been unable to replicate each others work, suggesting that there is a bias towards false positives (Earp and Trafimow, 2015). This problem certainly runs more widely in science than the study of human behaviour (Schooler, 2014). It has led to calls both for more rigorous experimental design and pre-registration of studies and more rigorous analysis and statistics. Whilst this will certainly go some way to improve scientific rigour this problem is a difficult one in a field which is characterised by its diversity of research approaches. For example, fitting the ethnographical approach of many anthropologists into such a framework is simply inappropriate. In my studies I aim to be as rigorously quantitative in my experimental design and analysis as modern approaches will allow.

1.3.2 Empirical Problems

Research has tended to rely on laboratory studies because it allows investigators to control the environment (Levitt & List, 2007). However, human behaviour is generally sensitive to contextual cues, meaning that the laboratory environment may alter cooperative behaviour (Haley and Fessler, 2005). Therefore there is a need to investigate cooperation in a field environment (Rankin, 2011) .

People in an experimental situation tend to behave in a way that they believe the experimenter wants (the experimental demand effect) (Barmettler, Fehr and Zehnder, 2012). Furthermore, when people know they are being observed they tend to behave more cooperatively (Bateson, Nettle and Roberts, 2006). Just the perception of being watched can change behaviour (Nettle, Nott and Bateson, 2012). Multiple examples suggest that when individuals are monitored they tend to work harder (Levitt and List, 2007). These behavioural shifts prevent accurate quantification of naturalistic cooperation.

Economic games carry no real risk and are inherently profitable as participants tend to be given their initial endowment. Many different economic games with varying pay offs are used (Rankin, 2011). There is sometimes a theoretical basis for the payoff, however this is rarely justified (Burton-Chellew and West, 2013). Generalising results from these studies is flawed by the combination of a lack of real costs and rationale for varying payoffs.

There is debate over the roles that between and within-culture variation play (Lamba and Mace, 2011; Henrich *et al.*, 2012). Cross-cultural analyses establish if laboratory results are generalizable when laboratory groups with different background cultures are used. High levels of cross-societal variation in punishing behaviour have been observed (Herrmann, Thöni and Gächter, 2008). The majority of studies of cooperation between non-relatives tend to use western undergraduates or online populations as their subjects. There has been little work examining whether this groups are representative (Benz and Meier, 2008). Absence of cross-cultural studies undermines much work in this field.

The problems with much of the theoretical and experimental work on human cooperation means that there is a need for experiments which replicate the structure of economic games in a naturalistic environment. Such experiments will establish if the conclusions drawn from laboratory studies of cooperation can be generalised to the real world and furthermore where discrepancies may lie.

1.4 What do we do?

I will address these problems by using a number of easily observed naturally occurring examples of cooperation, where it is unclear what drives cooperative and cheating behaviour.

In chapter two I will rigorously critique standard laboratory experiments. The simplest laboratory experiment is the dictator game where an experimental subject is given a small endowment and is invited to split this with an unrelated anonymous individual. I will use a simple but common analogue, pedestrians donating money to individuals begging on the street. Such individuals will be referred to as mendicants. I will critically assess the common laboratory finding that experimental subjects are highly prosocial by carrying out a number of experiments. I will compare the rate of donation in the street to the rate of donation in the laboratory. As laboratory subjects are normally given small endowments I will run a second study where I assess how much loose change pedestrians carry. I will then run an experiment where I give pedestrians endowments by dropping small sums of money in the street and recording pedestrians' donations make after picking this up. I will also run a laboratory style dictator game with pedestrians by stopping them in the street.

In chapter three I will extend this study of street dictator games. As most studies of dictator games occur in a laboratory context their scope to examine how social factors influence giving behaviour is limited. I will make a number of predictions about what influences pedestrians' donation behaviour based on the theory outlined above. I will ask whether the level of reciprocity in an interaction influences donation frequency. For example, do pedestrians give more to buskers than mendicants. I will also ask whether a number of reputational factors influence giving behaviour, for example, does group size or density of pedestrians influence donation frequency? I will also ask whether there are any sex differences in giving behaviour.

In chapter four I will study a naturally occurring public goods game, dog fouling in public parks. This easily observable, tractable system is an example of a public goods game as cooperation is costly at the individual level but benefits all park users. This system is particularly useful as it is possible to observe the level of cooperation, by assessing the amount of dog faeces present, without actually observing any individuals behaviour. I will ask firstly if property ownership affects how individuals make faeces abandonment decisions. We might expect that in areas with higher property ownership individuals will be more likely to cooperate as they have a greater stake in the upkeep of the area. I will also ask if the level of observation that individuals experience influences abandonment behaviour. I will go on to ask if dog walkers are sensitive to others behaviour in their abandonment decisions in this social dilemma by running an experiment where I manipulate the apparent level of dog faeces in the parks.

In chapter five I will use a second naturally occurring public goods game, cyclists at red lights. One of the strengths of the dog faeces study is that it is possible to measure cooperation without having to observe any individual decision-making. However, it is possible that an individual's context at the moment of decision-making is more important than general environmental variables. This study aims to address this. Jumping red lights is a public goods game as the time and energetic cost of cooperating accrues to individual cyclists whilst the benefit accrues to all road users. I will ask questions drawn from the theory discussed above as to whether the level of observation cyclists experience will change their likelihood of jumping and whether cyclists are influenced by the behaviour of other cyclists in making decisions about jumping.

Chapter 2 – Wild Dictator Games: The importance of context

2.1 Introduction

The frequency with which humans cooperate with anonymous non-relatives is an evolutionary conundrum, yet it is clearly fundamental to the existence and stability of human societies (Dunbar, 2003). Given the ubiquity of cooperative behaviour it is important to understand what mechanisms drive and maintain it (Stuart A. West, Griffin and Gardner, 2007). Any modern scientific investigation requires a rigorous empirical framework grounded in robust theoretical predictions (Earp and Trafimow, 2015). In order to understand this behaviour we therefore need: (i) A simplified, quantifiable framework that is empirically tractable and replicable; (ii) To remove assumptions and investigate what people actually do; and (iii) To have a basis in theory. Behavioural economists have achieved this by using economic games with a simplified pay off structure in a laboratory setting (Levitt & List, 2007). The results of these studies show that people are unexpectedly cooperative with one another. This has led to an underexamined consensus that people are highly prosocial (Wilson, O'Brien and Sesma, 2009). Laboratory games have several flaws: (i) People change their behaviour under observation; (ii) The experimental context itself can cause changes in behaviour; (iii) The pay-offs associated with experimental decision making aren't real; (iv) There is often a recruitment bias (Benz and Meier, 2008; Barmettler, Fehr and Zehnder, 2012; Burton-Chellew and West, 2013; Yoeli *et al.*, 2013). In order to critically evaluate this consensus of prosociality, it is therefore necessary to test the generalisability of laboratory games by identifying naturally occurring analogues and investigating whether people behave in similar ways.

One of the canonical games used by behavioural economists is the dictator game, which was developed by Kahneman to investigate fairness norms (Kahneman, Knetsch and Thaler, 1986). Kahneman *et al*. influentially concluded that people do not behave as 'rational' economic optimisers. The, now standard, format of the dictator game is that there are two players, a dictator and a recipient. These players are normally anonymous and unrelated to one another, although there are exceptions to this (Hoffman, Mccabe and Smith, 1996; Charness and Gneezy, 2008). The dictator is given an endowment and decides whether or not to share it, and how much to transfer to a recipient. If we assume that people are rational economic actors then we would hypothesize that the majority of participants would transfer nothing, however this is not what happens (Levitt & List, 2007). A relatively recent review showed that 40-60% of individuals choose to transfer part of their endowment to a recipient and that on average they transfer 20-30% of their initial endowment; this result is supported by a recent meta-analysis (Engel, 2011).

The dictator game has several potential flaws as a model for studying cooperative behaviour. They appear to be very unrealistic. The scenario for a subject of being asked a question about sharing small sums of money in a laboratory is not one that the majority of subjects will be familiar with in their day to day lives (Burton-Chellew & West, 2012; Levitt, & List, 2007). Given this, identifying naturally occurring analogues is a challenge (Burton-Chellew, El Mouden and West, 2016). In order to identify a rigorous analogue of the dictator game we need to be explicit about exactly what this interaction is. A dictator game describes a situation where an individual is invited to split an endowment with an unrelated, anonymous individual (Engel, 2011). Precisely this happens when pedestrians are being asked for change by mendicants in the street.

There has been limited investigation of donations to mendicants in the street, what there is has focussed on public perceptions of these individuals and their companion animals and on self-reporting of charitable behaviour (Lankenau, 1999; Kane, Green and Jacobs, 2010; Irvine, Kahl and Smith, 2012). One study suggested that the presence of panhandling in the US had a very limited effect on the public's attitude and behaviour towards mendicants (Lee and Farrell, 2003). In another study students were more likely to donate to a busker if they had previously donated to a mendicant, however, this study measured donation by self-reporting (Lemay and Bates, 2013). A further study linked perception and donation behaviour by asking whether mendicants with humorous signs received more donations; the reverse was found to be true (Boster *et al.*, 2016). There is one sociological study which examines specific interactions between mendicants and pedestrians and uses this to draw conclusions about the reciprocal nature of these interactions (Llewellyn, 2011). This study is marred by a small sample size and an entirely descriptive approach. Overall, I have not found any studies addressing the actual behaviour of individuals giving money to mendicants on a scale comparable to the present study.

There is a large body of evidence suggesting that people change their cooperative decisions when they are aware that they are under observation. The effect of "seeing eyes" has been well documented. When posters of both eyes and flowers were positioned next to an honesty box in a university tea room on alternate weeks, the rate of payment was higher when there were eyes (Bateson, Nettle and Roberts, 2006). This effect has been replicated causing decreases in both dog fouling and bike theft (Nettle, Nott and Bateson, 2012; Keep Britain Tidy, 2014). I demonstrate in chapters 3 and 5 that people are sensitive to crowd density when making cooperative decisions. This suggests that people care about what others think of their external image score or reputation (Nowak & Sigmund, 1998; Wedekind, 2000). I predict that people will behave more cooperatively when they are aware that they are under observation.

There is also evidence that individuals are sensitive to the laboratory context. One prior study has been carried out on a naturalistic dictator game. In this study, which took place in Las Vegas, experimental subjects at a bus stop were given a small number of gaming chips by an individual who claimed to be in a rush to get to the airport. There was always an experimental stooge loitering nearby. There were two treatments in the study: (i) The focal individual was given the chips by experimenter; (ii) The focal individual was given the chips by the experimenter who then suggested sharing them with the stooge. In contrast to the standard laboratory result none of the experimental subjects chose to split their endowment (Winking and Mizer, 2013). Not dissimilarly, differences in framing of experimental games can cause behaviour change. For example, in one study two groups of subjects played identical games called the "community" and "wall street" games. Those in the community game cooperated more than those in the wall street game, suggesting that subjects are sensitive to their experimental context (Liberman, Samuels, & Ross, 2004; List, Berrens, Bohara, & Kerkvliet, 2004). The most extreme examples of this can be seen in the Milgram and Stanford Prison Experiment (Milgram, 1963; Haney, Banks and Zimbardo, 1973). In the former, subjects believed that they were inflicting extreme amounts of pain on other experimental subjects under the instructions of the experimenter, whilst in the latter subjects behaving as "guards" exacted extreme punishment behaviour on subjects behaving as "prisoners". These studies all reveal that the experimental context is a highly specific one and individuals' cooperative decision-making may not generalise to normal life. I predict that when subjects are aware that they are in an experimental context they will behave more cooperatively.

Traditional economic theory assumes that people are rational economic actors who act to maximise their economic reward (Levitt and List, 2008). There is a well-established body of literature that suggests that this is not how people make decisions (Rankin, 2011). The standard laboratory dictator game is a classical piece of evidence for this. Further evidence that people are not rational economists comes from a study where individuals are put under time pressure to make cooperative decisions. Those under time pressure give away more of their endowment, suggesting that thinking about cooperation may carry a cognitive load (Rand, Greene and Nowak, 2012). Not dissimilarly, the discounting effect describes a situation where people value money in the future less than they do money now (Critchfield and Kollins, 2001). A further example of economic irrationality is how people spend windfalls. One study shows that people who did not anticipate a windfall go on to gamble more compared to controls, whilst an online study showed that people spend windfalls on groceries they would not normally purchase (Nash, Siegel-Jacobs and Stone, 1994; Milkman and Beshears, 2009). One anthropological study on the Amazonian Tsimane suggested that subjects share large windfalls more readily than small ones and that items with a higher level of acquisition variance are shared more readily (Gurven, 2004). These results lead me to predict that people will behave more cooperatively when they receive experimental windfalls.

In this chapter I will use the context of pedestrians giving money to mendicants in the street to ask if it is possible to generalise results from the laboratory dictator game to other contexts. I will observe the rate at which pedestrians give to mendicants and carry out several experiments where I give pedestrians small endowments. I hypothesise that pedestrians will give at a lower rate in the street context compared to the laboratory. I will anonymously give pedestrians small windfalls and predict that they will give more frequently when they are playing with these windfalls. I will also carry out an experiment were I stop pedestrians and invite them to play a formal dictator game on an iPad, I predict that this experiment will recapitulate the common lab result.

2.2 Methods

2.2.1 Observational Methods

Between 16/06/2014 and 25/02/2015 I observed 44493 pedestrians walking past focal mendicants in in five UK cities: Edinburgh, Glasgow, Oxford, Cambridge and London. I identified focal mendicants by systematically searching city centre streets choosing those who were actively asking for money and who could be observed discreetly. Active solicitation met the requirement for a true interaction between players, while discretion avoided potential observer effects and met the ethical need to avoid causing subjects distress. At the start of each observation session, I recorded the sex and estimated age of the mendicant; whether or not they had a dog; the presence of a collection cup or hat, whether or not this already contained money; and environmental variables (temperature, wind, precipitation).

During each observation session, I scored each pedestrian who passed the focal mendicant as 0 (did not donate) and 1 (donated). I also recorded the sex and estimated age of each pedestrian, whether they were alone or in a group (>1 person together); the sex ratio of any groups; and the presence of any children, prams or people in wheelchairs.

Observations were made in two formats: (i) with a pen and paper and (ii) automated on an iPad. The initial pilot observations and the first observations in the final dataset were made using datasheets and a pen and paper. I optimised this process by changing to using an iPad and the Animal Behaviour Pro application to collect data (Newton-Fisher, 2012). The same underlying data were collected in both protocols.

Observations were carried out on opportunistic basis for 3-4 weeks in each city location.

2.2.2 Money Carrying Survey

I carried out a *post hoc* survey to establish whether pedestrians carry loose change, In order to verify my assumption that pedestrians are indeed playing a form of dictator game with mendicants. A research assistant and I stood on pavements within 100 metres of previous observation sessions (in Edinburgh only). We asked all pedestrians if they had time to stop for a one question survey. Those who did stop were asked whether they were carrying change, and if so, how much. We surveyed 366 respondents across 9 observation sessions of 90 minutes each at 6 locations.

2.2.3 Money Drop Experiment

One of the key dissimilarities between the observational method outlined above and a conventional dictator game is that I did not give participants an endowment. I decided to carry out an experiment where I did give my street participants a small endowment. I did this by dropping small sums of money on the pavement "upstream" from a focal mendicant, outside either a coffee shop or bus shelter where a research assistant and I could stand without detection. We surreptitiously placed a 50 pence coin on the ground while pretending to tie up a shoelace. We observed the coin until it was picked up a pedestrian. We scored the finder as 1 (gave the coin to the focal mendicant) or 0 (kept the coin). As a control, we scored the behaviour of the next pedestrian walking in that direction. We also recorded whether the pedestrian who picked up the coin was walking toward or away from the mendicant. I carried out 58 trials in 5 observation sessions across 3 locations.

2.2.4 Dictator Game Under Observation

To examine the effect of an experimental observer, and to directly compare behaviour in a street with behaviour in a lab, I conducted a conventional dictator games in the street. I carried out a survey of participants' attitudes to homelessness in which the first question was a dictator game. I carried out 96 surveys at 3 locations.

Participants were offered a £5 endowment in the form on an Amazon voucher which would be emailed to them after the survey. They were then asked if they would like to split this endowment with the homelessness charity Shelter Scotland. Participants were able to keep the whole endowment or give any amount of it away in £1 increments. There were then several follow up questions on their attitudes towards homelessness and what their motivations for giving to mendicants would be (see Appendix 1). After the survey, the amazon vouchers were emailed out and appropriate donations made to Shelter Scotland.

The surveys were carried out by a research assistant and I using a standardised script (see Appendix 2), with responses recorded on iPads using Quick Tap Survey software ('Quicktap Survey Mobile Application v1', no date). The first screen of the survey was an information page which participants had to confirm they had read before continuing with the survey. All pages of the survey carried the University of Edinburgh logo. Participants had to give their consent to taking part before continuing. All participants were given a sheet with a follow-up email address, which they could use to contact after the survey to ask any further questions or withdraw their permission. Any personal data were kept on a laptop that had been encrypted and all data were anonymised prior to any further analysis. All surveys were carried out in a pair with another research assistant.

2.2.5 Analysis

Observational and Laboratory analysis

To compare the rate of giving in my street dictator game with the rate of giving in standard laboratory games I used the results generated by a relatively recent meta-study of dictator game giving (Engel, 2011, Figure 2.1). I calculated the number of giving events observed in this meta study (13297 out of a total 20813) and generated a contingency table, which also contained data from my observation of giving to mendicants. I then used a chi-squared test to establish if there was a difference in the rate of giving between these two different contexts.

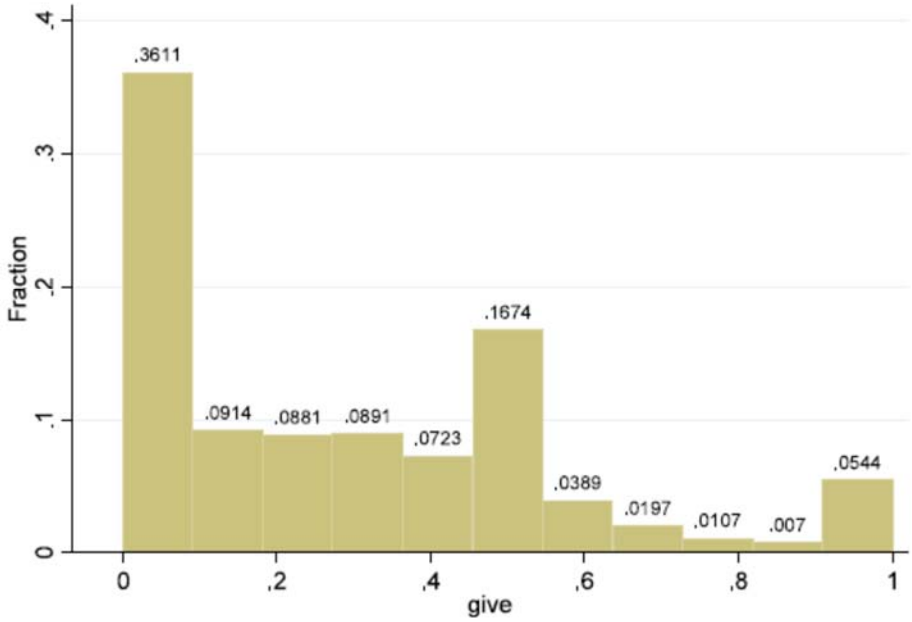

Figure 2.1 Distribution of individual give rates, taken from Engel, 2011, p589

Because all experiments in this study generated binary data, I made contingency tables for each experiment and then used chi-squared tests to establish if those classes were different to one another.

2.2.6 Ethics statement

All observational and experimental protocols were reviewed and approved by the School of Biological Sciences ethical review committee. Discreet observation is acceptable when all observations and experiments take place in public locations where members of the public have a reasonable expectation of being observed and all data are anonymised.

2.3 Results

2.3.1 Observed rates of donation

451 out of 44993 pedestrians (1%) donated to focal mendicants. This is a dramatically lower proportion of giving than is recorded in lab games (where 63.89% choose to split their endowment; χ^2= 34046, p< 2.2 x 10^{-16}; Figure 2.2). I tested my field data against the standard laboratory result. This was the appropriate comparison as I was interested in comparing behaviour observed in the laboratory to behaviour observed in a naturalistic setting.

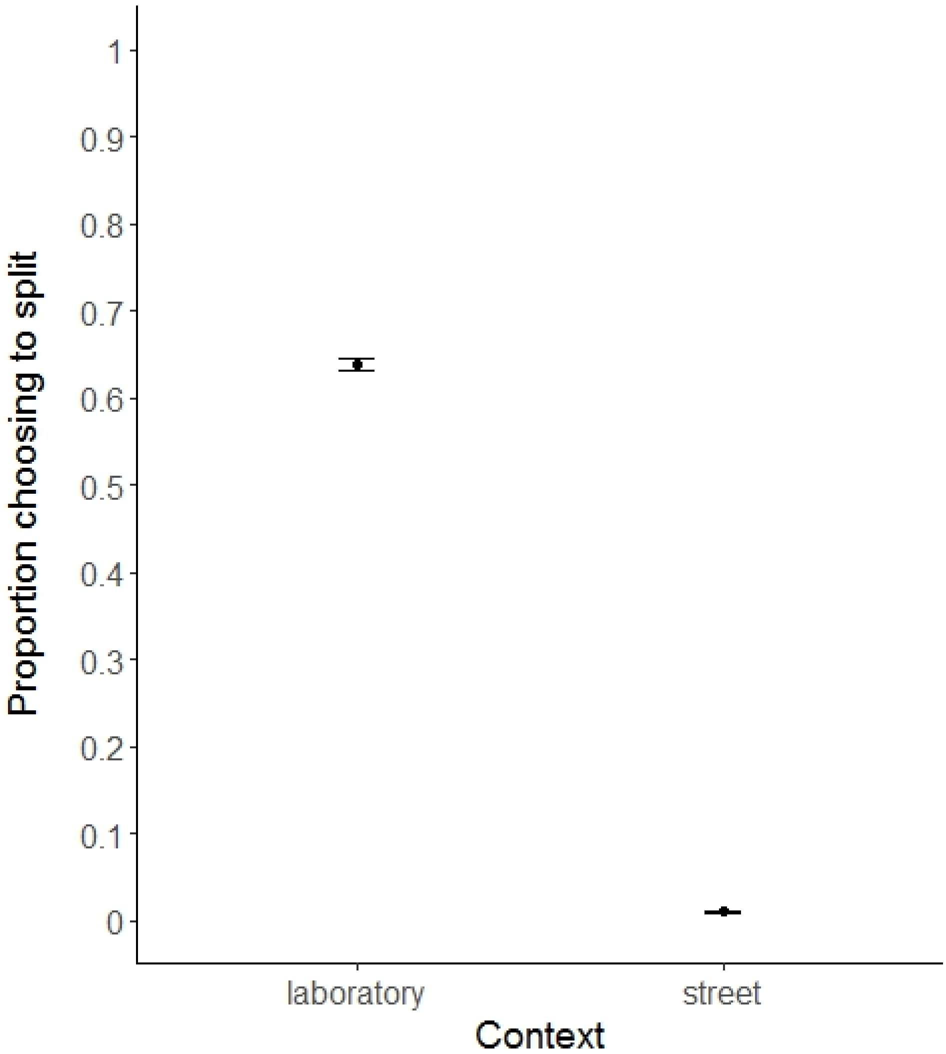

Figure 2.2 Proportion of participants giving in lab dictator games and a street analogue (donation to mendicants) (± binomial error bars; Laboratory N = 20813, Street N = 44993).

This difference in donation cannot be accounted for by low rates of change carrying alone: 46.9% of people surveyed were carrying change (median amount carried =£2, range 0-£12.50, Figure 2.3),

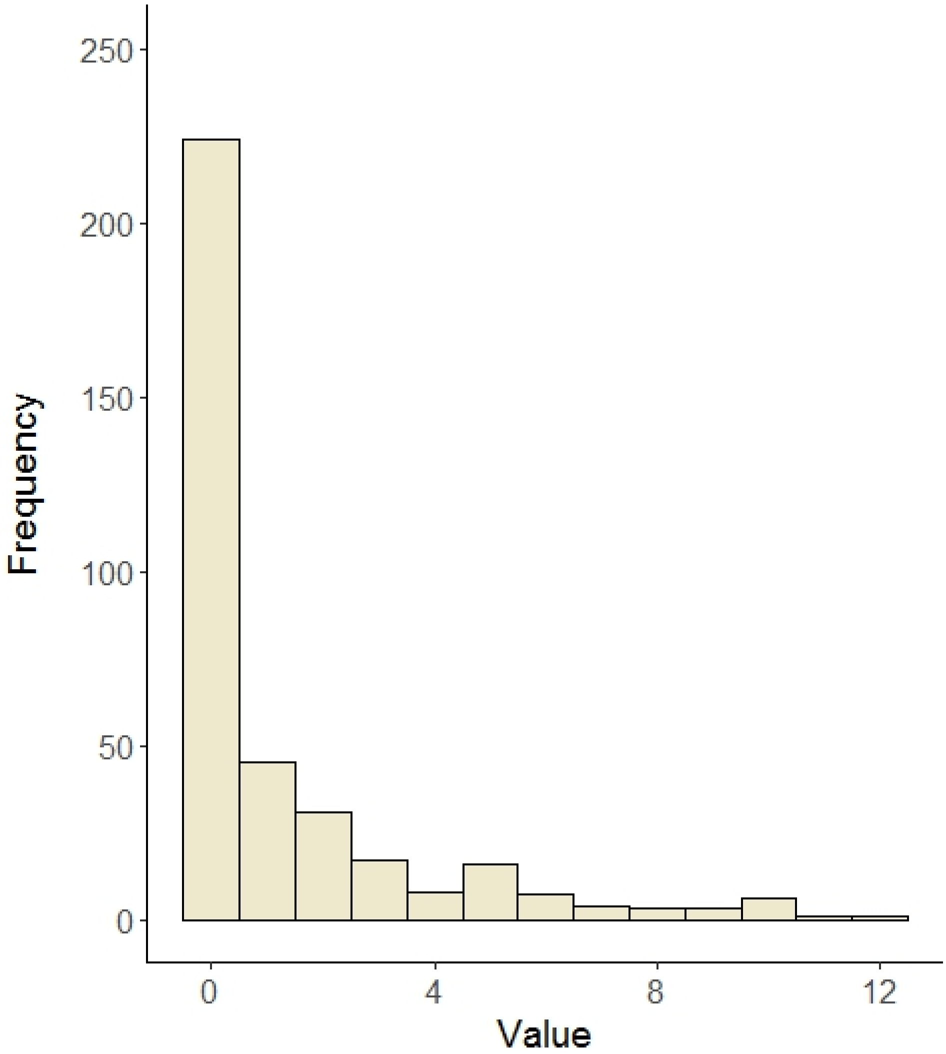

Figure 2.3 Change carrying histogram, Value in £, Frequency in number of people, N = 366

2.3.2 Money Drop Experiment

When I dropped money on the street and observed the behaviour of individuals that picked it up as they passed a mendicant, a higher proportion of individuals chose to donate their windfall (17.24%) than donated in the general observational dataset (~1%). The frequency of donation in the windfall treatment was also significantly higher than the frequency of donation in the explicit control (χ^2= 6.4277, p=0.011; Figure 2.4). The experimental treatment data were tested against the experimental control data collected at the same time. This comparison was appropriate as there was an ecologically valid control that allowed inferences to be drawn about individuals' behaviour when they had received a windfall.

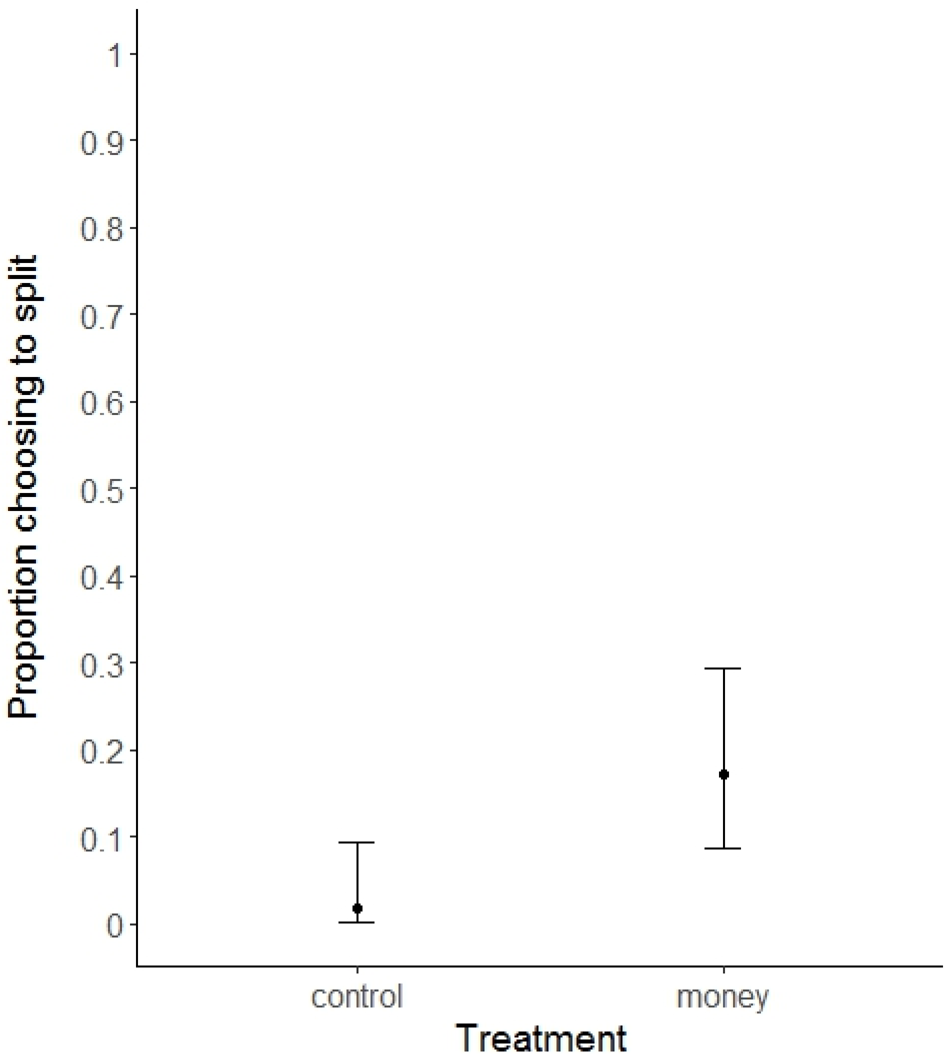

Figure 2.4 Money Drop Experiment Plot (± binomial error bars, N = 58)

2.3.3 Dictator Game Under Observation

When playing a dictator game on an iPad, while under observation by a researcher, significantly more people chose to split their £5 endowment with a Homeless Charity than to keep it all (χ^2=93.521, p< 2.2 x 10^{-16}; Figure 2.5, Figure 2.6). The two classes being tested against each other here is the proportion of respondents choosing to split the endowment versus the proportion choosing to keep it all. This comparison is appropriate here as this experiment had no formal control in the study design and demonstrates the difference in behaviour between these two groups.

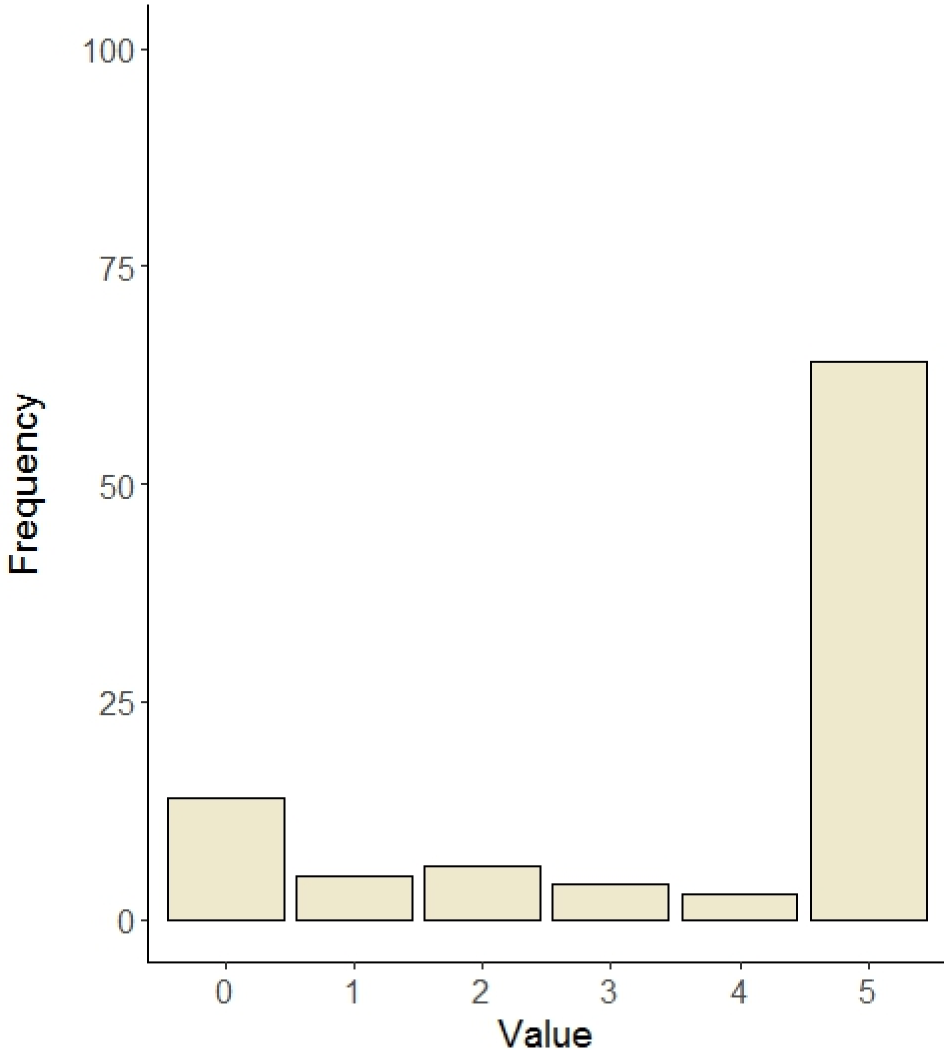

Figure 2.5 Histogram showing the values of endowment given away by experimental subjects in a dictator game carried out on an Ipad. The dictator game made up part of a survey of experimental subjects stopped in the street. Value of endowment given away in £, Frequency in number of people. N = 96

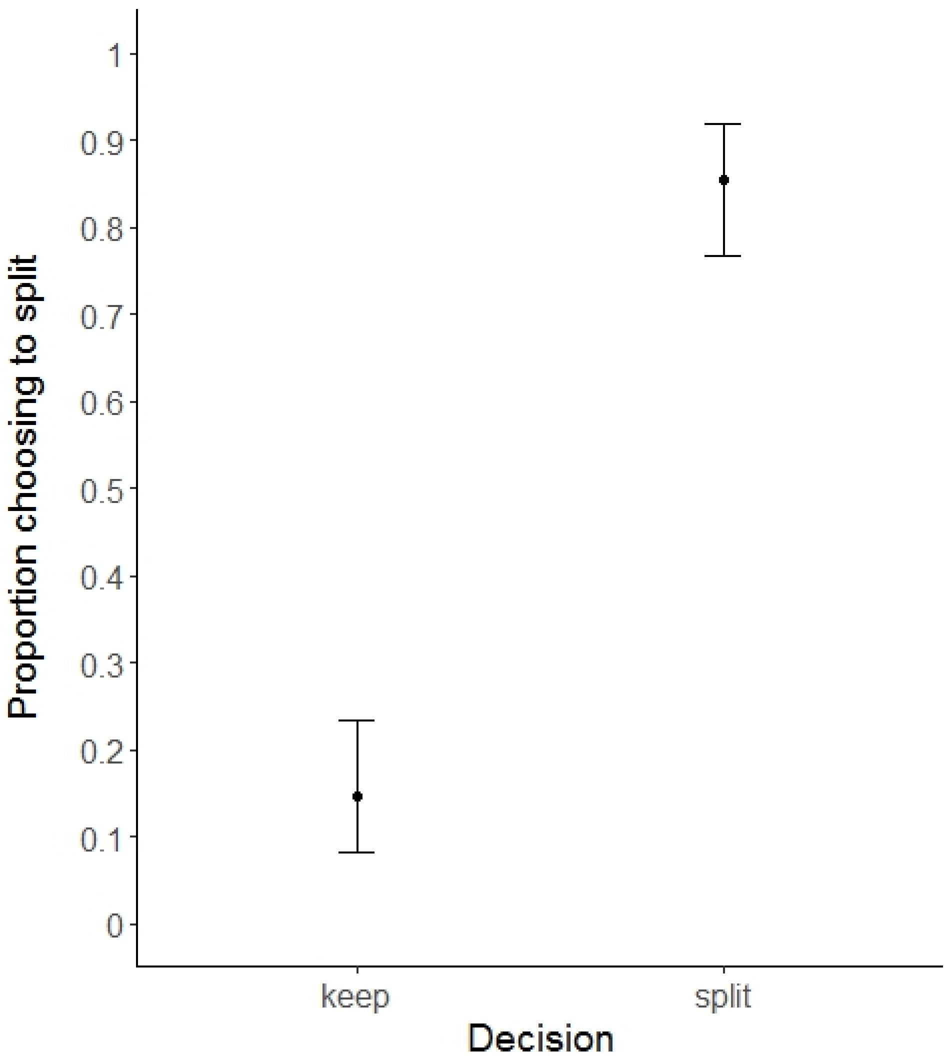

Figure 2.6 iPad dictator game (±binomial error bars. N = 96)

2.3.4 Comparison across contexts

There are differences between all treatments with giving occurring at a rate of 1%, 85%, 64% and 17% in mendicant, iPad, laboratory and money drop contexts respectively (Figure 2.7).

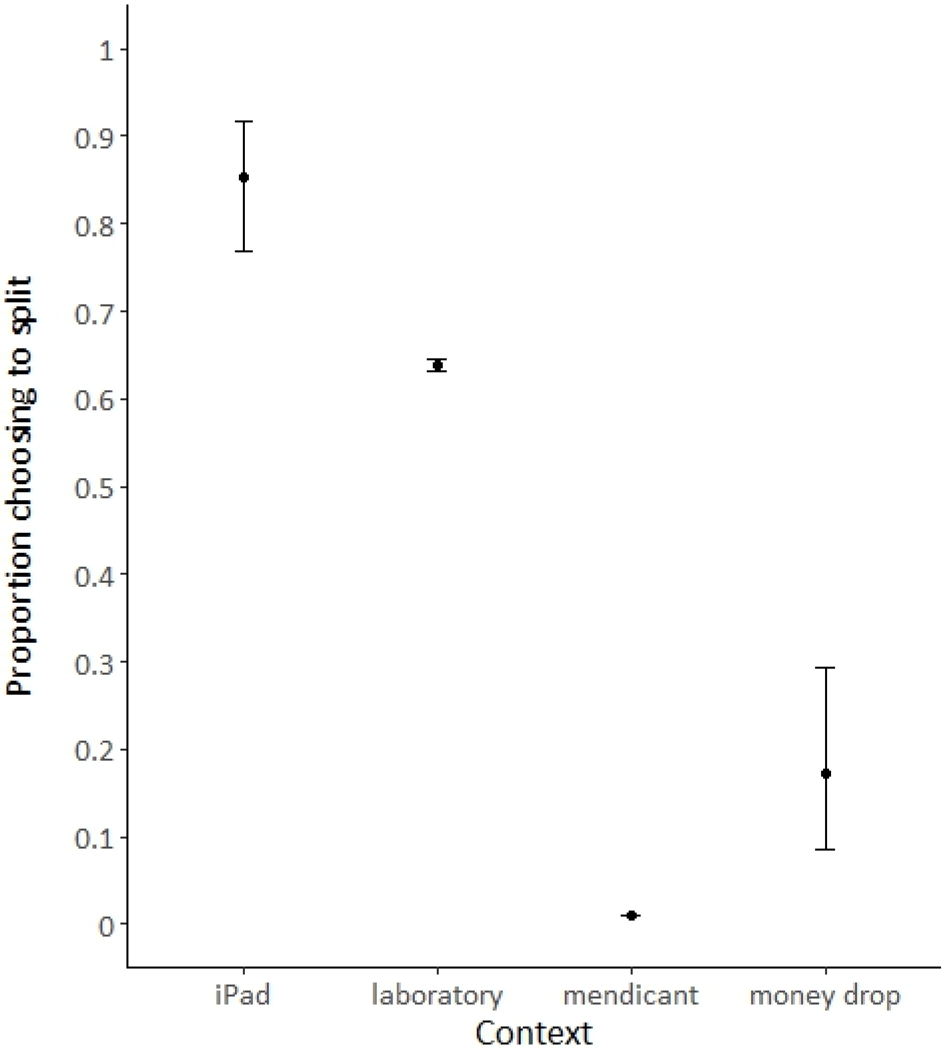

Figure 2.7 All experiment comparison plot (± binomial error bars. Mendicant N = 44993, iPad N = 96, Laboratory N = 13297, Money Drop N = 58)

2.4 Discussion

The first result in this chapter is the most striking (Figure 2.2). This result shows that there is a discrepancy between how people behaviour in the laboratory and the wild. Whilst there are certainly critiques that can be made of how strict the analogy between these two scenarios is, I address these in the follow up experiments in this chapter. This result shows that when people are invited to play a dictator game in the laboratory the majority of people will choose to split their endowment with an anonymous unrelated individual, yet when pedestrians are asked the same question by a mendicant in the street approximately 1% of people are willing to give away money.

There are several possible explanations for this result: (i) People have an intense awareness of their social scenario. Despite the anonymity that is a feature of the vast majority of laboratory games, subjects in these games cannot help but be aware of the fact that they are under observation. This result provides evidence that when there are similar pay-offs in different contexts people behave very differently and are sensitive to the fact that they are being observed. (ii) The existence of an experimental context has been shown to influence subjects' behaviour in several studies (Liberman, Samuels and Ross, 2004). It may well be the case that by comparing an experimental context to an observational one this difference is exacerbated. (iii) In the laboratory individuals are playing with endowments that they have just received as a windfall. From a rational economic perspective we might expect them to view this windfall money as their own, as you would money that you had earned that was in your pocket walking down the street. However, it is clear that people tend not to view windfalls in this way leading to the possibility that individuals are much more generous in the laboratory context than they would be with their own money (Nash, Siegel-Jacobs and Stone, 1994).

In my explanation of these results I have used the term pay-offs. In this context I do not mean purely the direct monetary benefits and costs that would be considered pay-offs in a economic setting. Instead I am considering the ultimate benefits and costs to an organisms fitness that an action can have. These benefits and costs could be quantified in the form of monetary value but they could also take other forms, like reputational benefits. My first result in this section indicates that when such pay-offs are held relatively constant but an individual's social context is changed then their cooperative decision changes. In this instance people are playing anonymously with similar sums so their ultimate pay-off is likely to be similar.

A further explanation for the low levels of giving seen in the street context is that people do not have change in their pockets. The rise in use of debit and credit cards to pay for goods and services may have led to a reduction of change carrying (*2016 UK Payment Markets*, 2016). I assayed this by carrying out a short study where I surveyed 366 individuals about whether they were carrying change and how much they were carrying (Figure 2.3). The result that around half of all individuals carry change leads me to believe that this is not a substantial barrier to giving. One study has shown that availability of change is important in dictator game giving (Fielding and Knowles, 2014). If I was to extrapolate from my first result and assume that giving would double if all individuals carried change, the proportion of individuals giving would only increase to ~2%, which in comparison to the laboratory result of ~63% remains a substantial difference.

Other explanations could include simply that people are simply distracted whilst walking along the street. In order for my study to be analogous to the laboratory dictator game I restricted my sample to mendicants' who were actively begging. Despite this, pedestrians may just not have noticed or paid attention to the focal mendicants. This is interesting as the role of attention in naturalistic cooperative behaviour has not been widely studied. There has been some work on small groups of children in primary schools and how they pay attention to each other (Gillies, 2003). A further experiment has examined how individuals level of experience at completing a screen-based task changes where they direct their gaze (Velichkovsky, 1995). However, the level of attention participants give may substantially change their cooperative decisions.

In laboratory dictator games individuals are given endowments to play with. I was interested in what would happen if small endowments were given in a street dictator game. People gave at a significantly higher level when they had picked up a small endowment (Figure 2.4). The most parsimonious explanation is that despite my critique of conventional economics, monetary pay-offs do matter. In this instance people have received a windfall of a small endowment and are much more willing to donate this money (Milkman and Beshears, 2009). This could be interpreted with the previous result as when pay-offs are held constant between contexts, context can have an impact on behaviour. However, when a pay-off is changed within a context, as in this experiment, the different size of pay-off can change behaviour. This is important as if laboratory results are to be generalised, cooperative behaviour will be very different depending on the source of their endowment. Furthermore, in real life scenarios, cooperative actions and pay-offs do not always take the form of money, they can also be goods, services or reputational benefits. Given this, thinking about what constitutes a windfall and how this might impact individuals' further cooperative decision-making could be an interesting direction for future research (Fielding and Knowles, 2014).

I wanted to test the hypothesis that observability changes behaviour in a dictator game. I ran a laboratory style dictator game on iPads in the street, sampling from the same population that I used for my other street studies. I found that people gave in this context at an extremely high rate (Figure 2.5, Figure 2.6). There could be several reasons for this: (i) In comparison to a laboratory games all subjects were asked to fill out their choice on an iPad in front of the experimenter. Although the rubric gave a reassurance that all data were treated anonymously the subjects could not help but be aware that someone was standing immediately in front of them and asking if they wanted to donate. This may have influenced their decision. (ii) In this study individuals were asked if they wanted to split the money with a charity as opposed to a particular individual. There is evidence that there are social injunctions against giving to mendicants (Phelan *et al.*, 1997). It may be the case that people were simply more willing to give to a homelessness charity as instead of a mendicant. When this result is compared to the laboratory game it could also be the case that people were more willing to give to a charity, which may be seen as more deserving, than another anonymous subject (Reinstein and Riener, 2012).

The distribution of giving in the iPad dictator game also differs from that seen in laboratory games (Figure 2.1, Figure 2.5). The key difference being that in the iPad game the majority of those that gave, donated their whole endowment rather than splitting it, although the subjects were given this option. There is evidence that under time pressure people tend to behave in a more cooperative way (Rand, Greene and Nowak, 2012). People might feel like they are under time pressure in this setting resulting in a higher level of donation. Alternatively, individuals may feel that the value of the windfall they are receiving is not worth the hassle of giving away their email address and subsequently reclaiming a voucher worth less than five pounds.

Overall, this study shows that there are clear differences in how people go about making cooperative decisions when they are not in a laboratory. The follow-up experiments then go on to show unambiguously that both pay-offs to the individual and the level of scrutiny the individual believes themselves them to be under are important in cooperative decision-making. Finally, there is some evidence that supports the idea that people will change their cooperative decision when under time pressure. These results are important as they provide evidence that context is important in cooperative decision-making and that the value of pay-offs does change individuals' decisions. More broadly this allows us to question the results drawn from laboratory games and their general conclusion that humans are unexpectedly prosocial.

Chapter 3 – Do social environments influence giving in dictator games?

3.1 Introduction

The high frequency of human cooperative behaviour between non-relatives poses a dilemma. This cooperation is relatively stable despite constant selection for cheating. There are several mechanisms which stabilise cooperation, chiefly reciprocity and punishment (Fehr and Gächter, 2002; Gintis and Fehr, 2012). In reciprocity, interactants provide sequential benefits to one another. Reputational information can stabilise these interactions as individuals can choose to interact selectively with those with good reputations whilst avoiding or punishing those with poor reputations (Falk and Fischbacher, 2006; Sylwester and Roberts, 2010). We would therefore expect individuals to adjust their social behaviour to maintain a good reputation (Nowak & Sigmund, 2005). However, cooperative behaviour can also be costly. Evolutionary theory predicts individuals will act selfishly to minimise their costs, therefore we would expect individuals to optimise reputational benefits whilst minimising costs (Hamilton, 1964).

As studies of dictator games generally occur in the laboratory it has not been possible to examine if such reputational benefits influence naturalistic cooperative behaviour, as laboratory studies normally require that participants' decisions are anonymised. The analogue of giving to mendicants in the street allows these questions to be studied explicitly. We might expect individuals to behave more prosocially in the presence of others in order to maintain a cooperative reputation. A phenomenon known as an audience effect (Hamilton & Lind, 2016; Triplett, 1898). This could lead to a prediction that as, for example, donation visibility increases there will be an increase in donation rate (Andreoni and Petrie, 2004; List, 2006; Jones and Linardi, 2012). However, some studies suggest that as a behaviour's visibility increases, individuals adopt the local social norm, which could be cooperative or non-cooperative, this effect is stronger in women. Adopting a local norm may be a low risk reputational strategy. (Jones and Linardi, 2012; Exley, 2016). This effect is mirrored in work which suggests that reputation effects tend to narrow the set of possible equilibrium distributions, suggesting that people tend to conform to a particular set of behaviours (Levine, 1996). Overall I predict that higher visibility will lead to more donations, so individuals will give more in larger groups and when there is a higher density of pedestrians.

Reciprocity will also affect the relationship between donors and recipients. When a donor gives to a mendicant, there is not generally an expectation of an ongoing reciprocal relationship. However, donations to buskers can be regarded as payment for music. This leads me to expect that buskers will receive higher levels of donation than mendicants. As there is no reciprocity present with charitable collectors I would also expect them to receive less than buskers. However, as there is no social stigma associated with giving to charitable collectors, I predict that collectors will receive more than mendicants (Belcher and DeForge, 2012).

Social contagion, where observers adopt the behaviour of others; (Kearns *et al.*, 2009; Christakis and Fowler, 2010; Tsvetkova and Macy, 2014). can influence both prosocial (Van baaren *et al.*, 2004; Shang and Croson, 2009) and antisocial behaviour (Faria, Krause and Krause, 2010). Broadcasting of prosocial behaviour tends to result in observers engaging in more prosocial behaviour (Galaskiewicz and Burt, 1991; Fowler and Christakis, 2010; Lacetera, Macis and Mele, 2015). However, it is unclear whether these effects are contagion or homophily, where similar people are more likely to be friends and behave similarly (Mcpherson, Smith-lovin and Cook, 2001; Leider *et al.*, 2009). Moreover, Social contagion is not universal: Tsvetkova &Macy (2014) suggest that whilst receiving help may lead to recipients engaging in more helping behaviour, observing someone else helping may lead to observers being less generous. One online study suggested that individuals may falsely broadcast their prosocial behaviour, however even these false broadcasts went on to cause increases in others prosocial behaviour (Lacetera, Macis and Mele, 2015). The general result that individuals tend to copy social behaviour leads me to predict that pedestrians who observe donations would be more likely to donate themselves.

Sex differences in charitable behaviour are well established (Einolf, 2011; Böhm and Regner, 2013). Women donate more than men, although there is debate about whether men give larger amounts when they do give, and the effect of marital status is unclear (Piper and Schnepf, 2008). Women are almost twice as likely to give to causes focussed on wellbeing such as, healthcare, homelessness and education charities (Marx, 2000). Men are more likely to give to charities involved in sport or civil rights (Einolf, 2011). There have been explanations for these results, with women reporting a greater sense of responsibility and empathy towards those less fortunate, whilst men are more likely to give when there is an opportunity for reputational gain (Böhm and Regner, 2013). Several studies have indicated that men and women's giving changes when framed in different ways (Chowdhury, Jeon and Saha, 2017). One study indicated that a higher proportion of men give to mendicants than women. this study was substantially smaller than the present one (Goldberg, 1995). The majority of work suggests that women give at a higher rate than men and I would predict that to be the case in the present study.

There is limited research addressing the factors influencing pedestrians giving on the street. What little research there is tends to focus on individuals' perceptions of mendicants and their companion animals (Lankenau, 1999; Kane, Green and Jacobs, 2010; Irvine, Kahl and Smith, 2012). One study suggested that the presence of panhandlers in a local area in the US didn't change the public's attitude to panhandlers (Lee and Farrell, 2003). In another study students said they would be more likely to give to a busker if they had also previously given to a mendicant, however, this study relied on self-reporting (Lemay and Bates, 2013). One study made a link between perception and behaviour by asking whether mendicants with humorous signs received more donations. The reverse was found to be true (Boster *et al.*, 2016). A sociological study examined interactions between mendicants and pedestrians and drew conclusions about their reciprocal nature (Llewellyn, 2011). This study is marred by a very small sample and an entirely descriptive approach. Overall, I have found a paucity of studies addressing the actual behaviour of individuals giving money to mendicants, buskers or charitable collectors on a scale comparable to mine. One large-scale study of more generalised helping behaviour in 36 US cities found that individuals' helping behaviour has a negative correlation with population density. This study also highlighted the need for measures of the total number of pedestrians passing by as well as those helping, which it did not report (Levine *et al.*, 1994).

This study examines the social influences on donation behaviour in the street. I ask: (i) Does group composition affect giving – and in particular does group size or sex ratio affect donation? (ii) Do people experience social contagion in giving – and in particular does giving by others affect donation? (iii) Does recipient type affect donation – and in particular do people give more frequently to charitable collectors or buskers than mendicants? And (iv) Are there sex differences in giving behaviour?

3.2 Methods

3.2.1 Observational Methods

Between 16/06/2014 and 25/02/2015 I observed 66994 pedestrians walking past focal mendicants, 21301 pedestrians walking past buskers, and 10474 pedestrians walking past charity collectors in 5 cities. This gave a total of 98769 observations of pedestrians. See Chapter 2 for focal mendicant selection criteria and data collection protocol. To select buskers and charity collectors, I systematically searched city centres and chose buskers who were performing and charitable collectors who were shaking tins. I combined these datasets, see Appendix 3 for data configuration script. This generated a data set consisting of 66761 observations of groups walking past mendicants, pedestrians and buskers that could be subdivided in specific ways to address my questions. Note that this dataset consisted of 66761 groups of pedestrians as opposed to groups of pedestrians.

3.2.2 Analysis

3.2.2.1 Do group size and sex ratio affect donation?

Using only data collected from mendicants, I constructed a binomial generalised linear mixed model (GLMM; 1=donation, 0 = no donation). I fitted group size as a factor (1 or >1); and group sex ratio (proportion of females in a group) as a continuous variable. I fitted Session, Location and Site as random terms. For additional variables investigated, see Table 3.1.

3.2.2.2 Does others' donation behaviour affect current donation rate?

Using a subset of the data on mendicants where the exact timing of each event had been recorded (see Appendix 3 for explanation), I constructed a binomial GLMM (1=donation, 0=no donation), with donation during the preceding two minutes (0 = no donation, 1 = at least one donation) as the fixed effect of interest. To control for variation in the number of people in the street, I also fitted crowd density (number of pedestrians in previous two minutes) as a fixed effect. I fitted Session, Location and Site as random terms. For additional variables investigated, see Table 3.2.

3.2.2.3 Does recipient type affect donation?

Using the full data set, I constructed a binomial GLMM (1=donation, 0 = no donation), with recipient type (mendicant, busker, charitable collector) as the fixed effects of interest, and Session, Location and Site as random terms (see Table 3.3 for additional terms tested).

3.2.2.4 Does sex affect donation?

Using only data collected on solitary pedestrians, and where the exact timing of each event was known, I constructed a binomial GLMM (1=donation, 0 = no donation), with pedestrian sex as the fixed effect of interest. I fitted Session, Location and Site as random terms. (see Table 3.4 for additional terms tested).

I also examined the interaction of sex of donor with sex of recipient (see Table 3.5)

All analyses were carried out in the lme4 package of R version 3.3.1, RStudio 1.0.136.

3.2.3 Coffee Buying Experiment

To further investigate the effect of previous donation on current behaviour (‘social contagion’), we manipulated the social environment by conducting 63 experimental coffee donations to mendicants in Edinburgh city centre, between 01/10/2015 and 30/11/2015.

Once a focal mendicant had been identified, a research assistant and I recorded pedestrian behaviour for 10 minutes, using the standard protocol. After 10 minutes, I approached the focal and asked if I could buy them a hot drink. If they said yes, I bought the drink, gave it to them, and re-joined my assistant. The assistant recorded the time of asking and giving of a hot drink. We then recorded pedestrian behaviour for 10 minutes after the hot drink donation.

I compared the proportion of pedestrians donating in either the 5 or 10 minutes before and after the experimental donation, using Wilcoxon-signed rank paired tests.

3.2.4 Ethics statement

All observational and experimental protocols were reviewed and approved by the School of Biological Sciences ethical review committee.

3.3 Results

3.3.1 Do group size and sex ratio affect donation?

Pedestrians in groups were less likely to give than solitary pedestrians (Table 3.1, Figure 3.1). People in groups with a larger proportion of males were marginally more likely to give (Table 3.1).

Table 3.1 GLMM examining whether group size and sex ratio affect the probability that an individual pedestrian donates to a mendicant (n=66994 pedestrians from 88 observation sessions in 40 locations on 48 days)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	0.014 (0.011, 0.019)	-28.465	$< 2.0 \times 10^{-16}$
Group Size (1 or >1)	0.518 (0.415, 0.641)	-6.105	1.03×10^{-9}
Sex Ratio	0.835 (0.683, 1.021)	-1.793	0.0729
Money	0.904 (0.652, 1.258)	-0.620	0.5351
Rain: Light	1.223 (0.726, 2.038)	0.785	0.4324
Rain: Heavy	1.077 (0.308, 3.324)	0.126	0.8998
Random Effect	**Variance**	**Standard Deviation**	
Session	0.1649	0.4061	
Date	0.1470	0.3824	
Location	0.1171	0.3421	

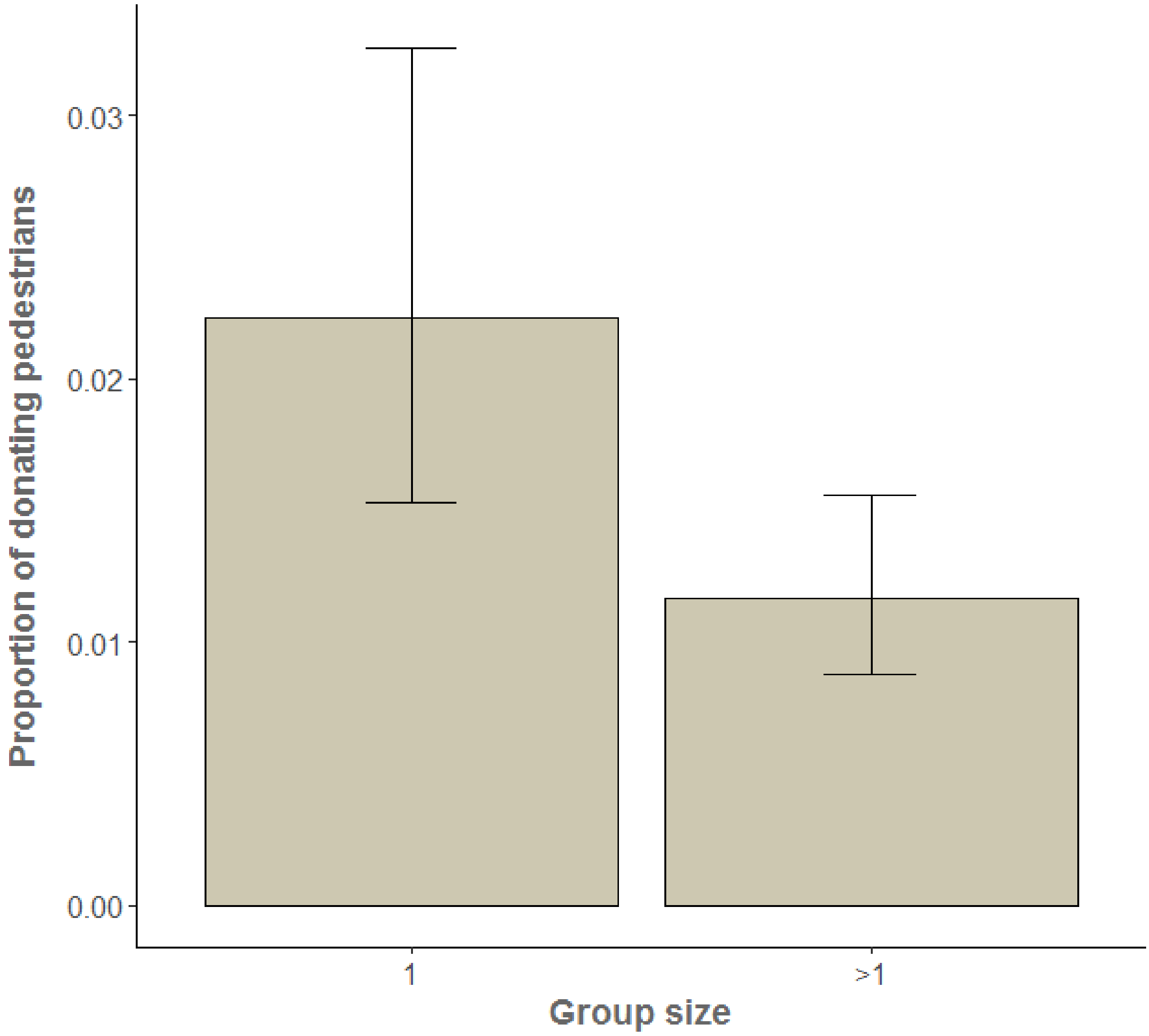

Figure 3.1 The effect of group size on probability of donating to a mendicant (model estimates ± 95% Confidence Intervals)

3.3.2 Does others' donation behaviour affect current donation rate?

Donation by another person during the preceding two minutes had no effect on likelihood of donation (Figure 3.2, Table 3.2). However, the probability of donation declined as pedestrian density increased (Figure 3.2, Table 3.2).

Table 3.2 GLMM examining whether donation by others affects the probability that an individual pedestrian donates to a mendicant (n=65926 pedestrians from 111 observation sessions in 52 locations on 36 days).

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	0.060 (0.031, 0.113)	-8.871	2×10^{-16}
Group Size	0.393 (0.284, 0.534)	-5.927	3.08×10^{-9}
Sex Ratio	0.883 (0.671, 1.162)	-0.904	0.3661
Rain: Light	1.257 (0.686, 2.293)	0.762	0.4459
Rain: Heavy	0.310 (0.346, 5.727)	0.407	0.6837
Crowd Density	0.986 (0.972, 1.000)	-2.079	0.0376
Prior Donation	1.200 (0.942, 1.495)	1.548	0.1216
Random Effect	**Variance**	**Standard Deviation**	
Session	0.127843	0.35755	
Date	0.136225	0.36909	
Location	0.001611	0.04014	

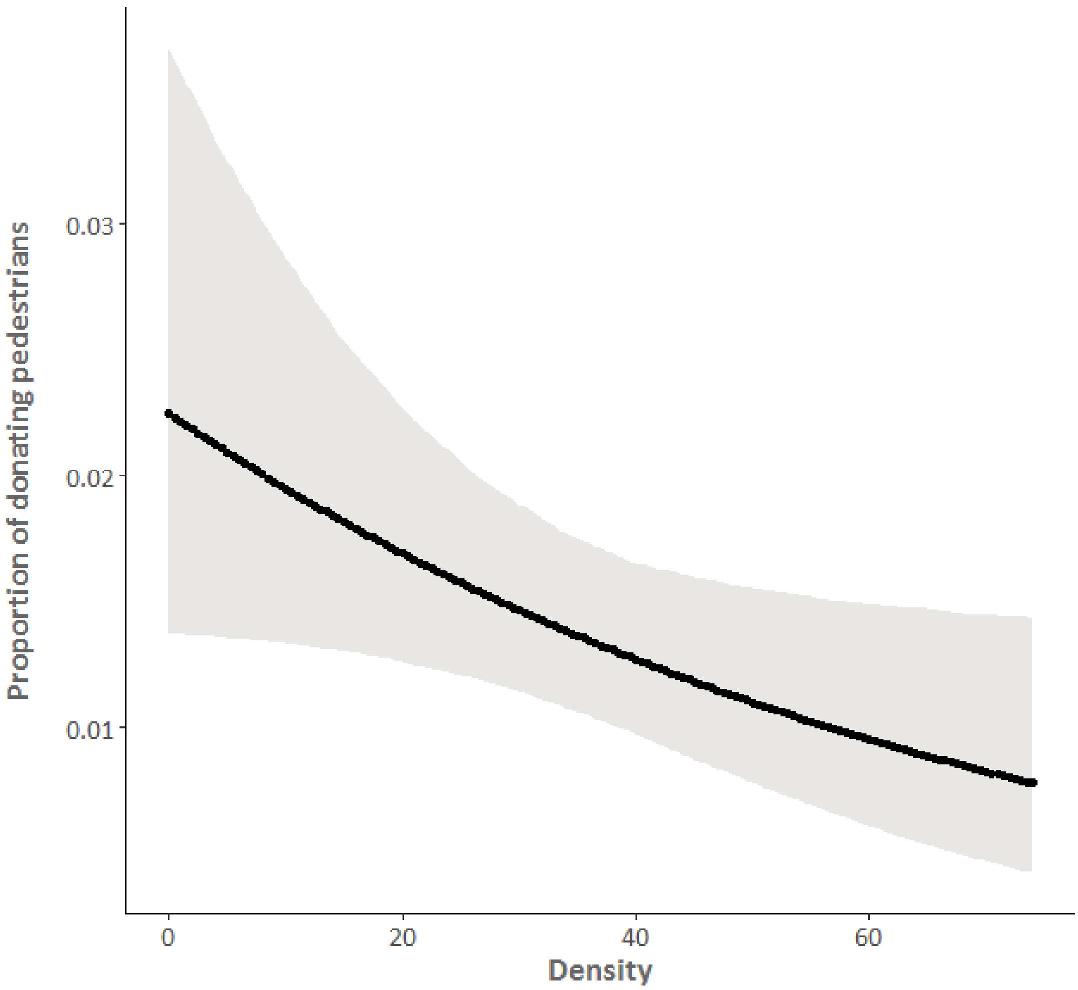

Figure 3.2: The effect of crowd density (people per 2 min) on probability of donating to a mendicant (model estimates ±95% Confidence Intervals).

3.3.3 Does recipient type affect donation?

Mendicants received the fewest donations, followed by buskers, then charitable collectors (Table 3.3, Figure 3.3 The effect of recipient type on probability of donating to a mendicant (model estimates ±95% Confidence Intervals).).

Table 3.3 GLMM examining whether recipient type affects the probability that an individual pedestrian donates (n=98769 pedestrians from 141 observation sessions in 65 locations on 54 days)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	0.034 (0.025, 0.044)	-24.883	2×10^{-16}
Group Size	0.489 (0.427, 0.558)	-10.751	2×10^{-16}
Sex Ratio	1.098 (0.971, 1.243)	1.514	0.130
Rain: Light	1.204 (0.863, 1.678)	1.123	0.261
Rain: Heavy	1.324 (0.391, 3.866)	0.500	0.617
Type: Charity	1.139 (0.724, 1.784)	0.579	0.563
Type: Mendicant	0.370 (0.270, 0.509)	-6.288	3.22×10^{-10}
Random Effect	**Variance**	**Standard Deviation**	
Session	0.17915	0.4233	
Date	0.04575	0.2139	
Location	0.15629	0.3953	

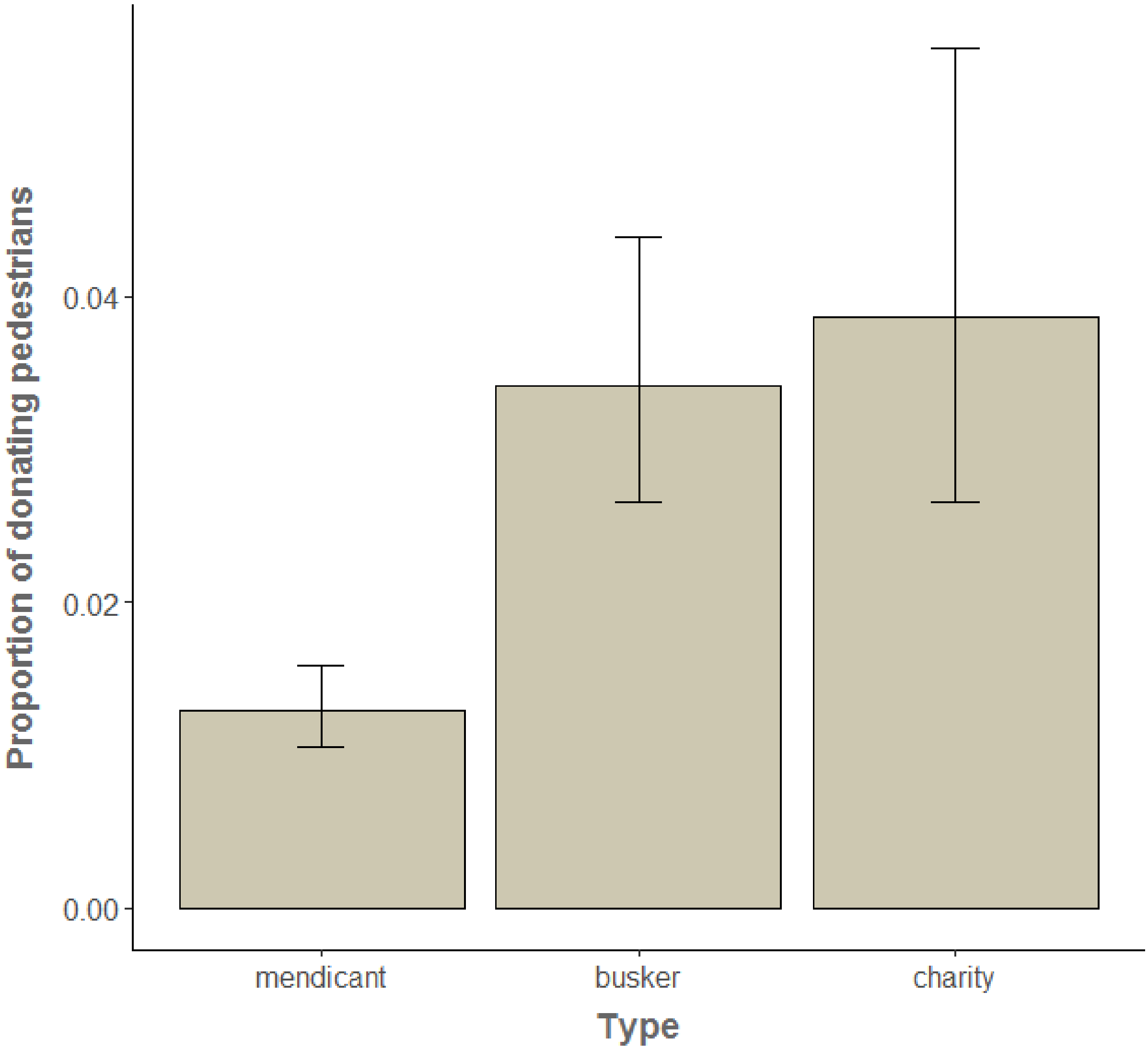

Figure 3.3 The effect of recipient type on probability of donating to a mendicant (model estimates ±95% Confidence Intervals).

3.3.4 Does sex affect donation?

Sex appeared to affect probability of donation, with men giving at 82% the rate of women (Table 3.4, Figure 3.4), with no interaction between pedestrian sex and recipient type ($z = -1.59$, $p = 0.11$).

Table 3.4 GLMM examining whether the sex of solitary pedestrians affects the probability of donating to all recipient types (n=26489 pedestrians from 111 observation sessions in 52 locations on 36 days)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
Intercept	0.052 (0.033, 0.083)	-12.669	$< 2 \times 10^{-16}$
Pedestrian Sex: Male	0.820 (0.709, 0.947)	-2.737	0.0062
Focal Sex: Male	1.000 (0.689, 1.445)	-0.002	0.9981
Type: Charity	1.184 (0.761, 1.826)	0.773	0.4394
Type: Mendicant	0.438 (0.313, 0.616)	-4.898	9.67×10^{-7}
Rain: Light	0.998 (0.693, 1.430)	-0.011	0.9909
Rain: Heavy	1.389 (0.336, 5.731)	0.474	0.6352
Crowd Density	0.978 (0.961, 0.995)	-2.562	0.0104
Random Effects	**Variance**	**Standard Deviation**	
Session	0.09551	0.3090	
Location	0.13557	0.3682	
Date	0.05646	0.2376	

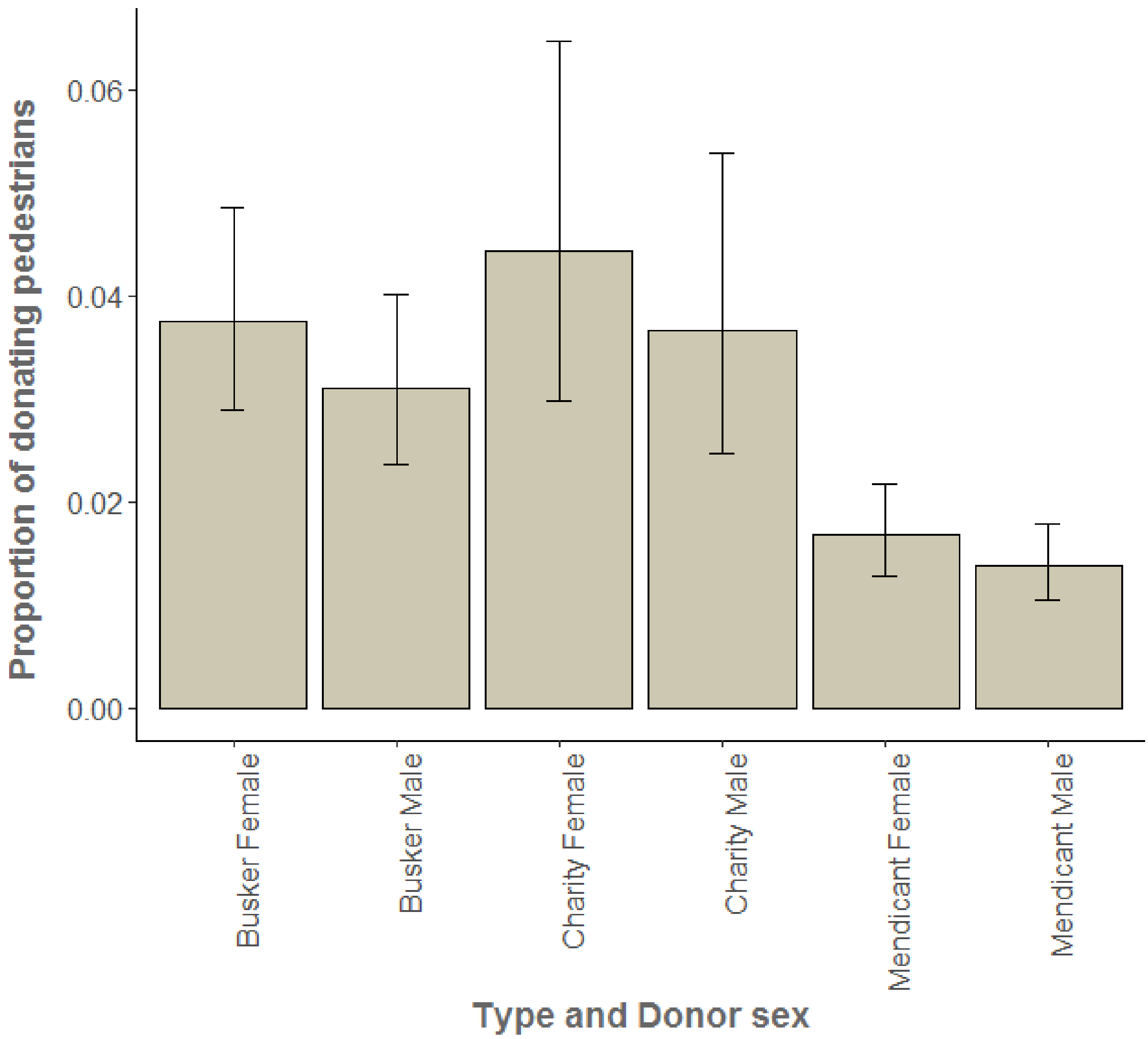

Figure 3.4 The effect of pedestrian sex donating to all types of recipient (model estimates ±95% Confidence Intervals).

To further investigate the sex effect and ensure that it was not an artefact driven by idiosyncrasies or uneven sample sizes within the data for the different recipient types, I re-analysed the data from each recipient type separately. There was a strong effect of pedestrian sex when giving to charitable collectors, with men giving at 69.9% the rate of women (GLMM, odds ratio = 0.699 (95% CI=0.538- 0.902), z=-2.74, p=0.0062). However, there was no sex difference when giving to buskers (GLMM, odds ratio = 0.830 (95%CI=0.658-1.044), z=-0.60, p=0.11) or to mendicants (GLMM, odds ratio = 0.958 (95%CI=0.736-1.248), z=-0.32, p=0.75). See Appendix 4 for full model outputs.

Table 3.5 GLMM examining whether the sex of solitary pedestrians affects the probability of donating to all recipient types, including an interaction of pedestrian sex and recipient sex (n=26489 pedestrians from 111 observation sessions in 52 locations on 36 days)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
Intercept	0.0454 (0.027, 0.074)	-12.398	$< 2 \times 10^{-16}$
Pedestrian Sex: Male	1.119 (0.771, 1.627)	0.603	0.5464
Focal Sex: Male	1.192 (0.782, 1.826)	0.828	0.4078
Type: Charity	1.185 (0.761, 1.826)	0.779	0.4360
Type: Mendicant	0.438 (0.313, 0.616)	-4.905	9.32×10^{-7}
Rain: Light	0.997 (0.693, Inf)	-0.016	0.9871
Rain: Heavy	1.399 (0.338, 5.393)	0.486	0.6271
Crowd Density	0.978 (0.962, 0.995)	-2.565	0.0103
Pedestrian Sex: Male * Focal Sex: Male	0.693 (0.462, 1.038)	-1.812	0.0700
Random Effects	**Variance**	**Standard Deviation**	
Session	0.09635	0.3104	
Location	0.13460	0.3669	
Date	0.05594	0.2365	

3.3.5 Coffee buying experiment

Experimental donation to a focal mendicant had no effect on donation by pedestrians, regardless of whether the comparison period was either five minutes (Wilcoxon, $V_{63} = 191$, p = 0.27) or ten minutes (Wilcoxon, $V_{63} = 578.5$, p = 0.74) before and after an experimental donation (Figure 3.5, Figure 3.6).

reverse

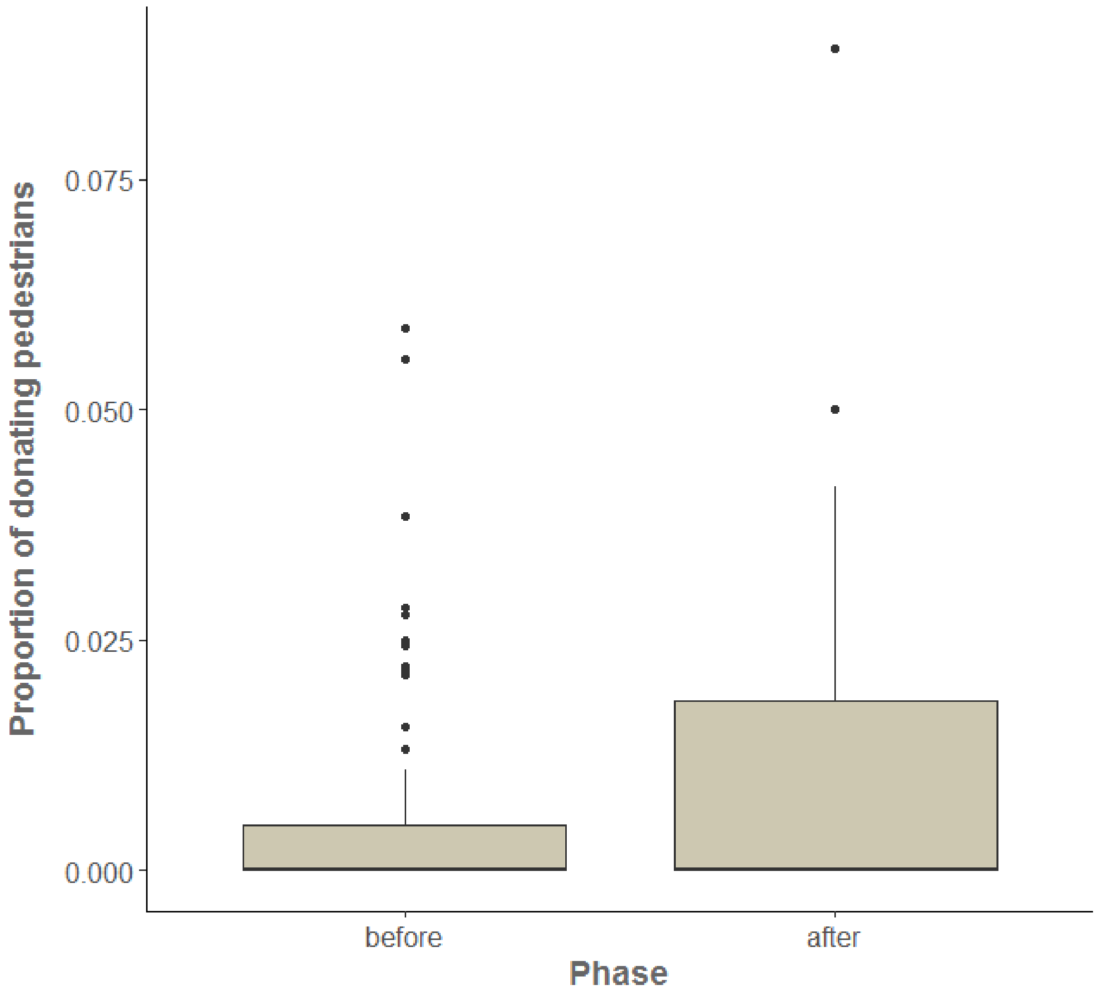

Figure 3.5 The proportion of pedestrians donating five minutes before and after an experimental donation to a focal mendicant. N=63

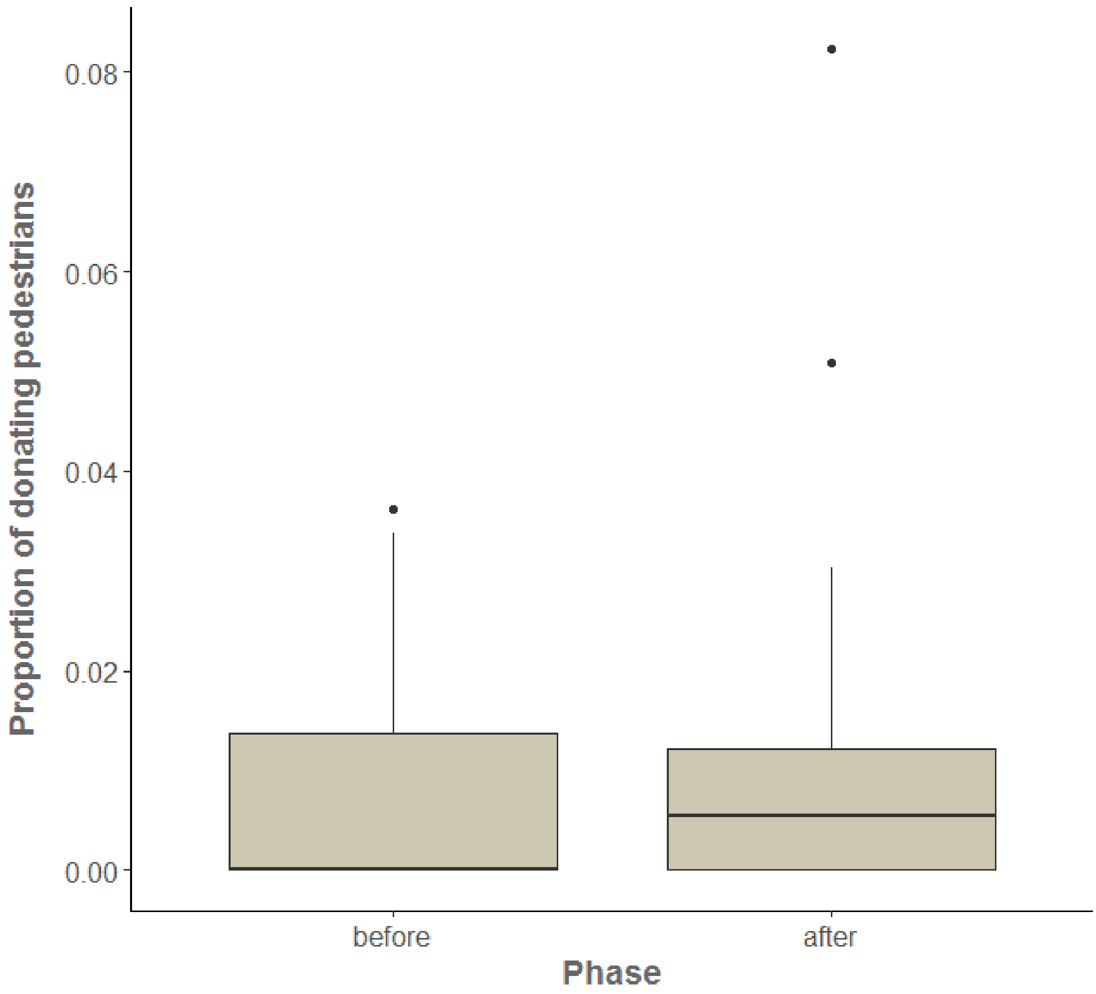

Figure 3.6 The proportion of pedestrians donating 10 minutes before and after an experimental donation to a focal mendicant. N=63

3.4 Discussion

My first two hypotheses relate to the effect of group size and density of pedestrians in the street. I predicted, based on the literature and evolutionary theory that people would give more when their donations were more visible. So individuals in larger groups and in crowds would be expected to give at a higher rate. My results do not support these hypotheses. Firstly, people give more when they are on their own (Figure 3.1, Table 3.1). This result is a replication of a much smaller previous study (Goldberg, 1995). There could be multiple reasons for this. For the group size result, although evidence suggests that people are more likely to produce prosocial signals when under observation (Andreoni and Petrie, 2004; Nettle, Nott and Bateson, 2012), there are also studies suggesting that under observation people were more likely to obey whatever they believe to be the social norm (Jones and Linardi, 2012). As I have shown in chapter 1, giving in this context is a rare event, meaning that in the majority of groups giving does not occur, which means there is a consistent social norm of “don’t give”. Giving to mendicants can be a socially sensitive issue and people in groups might be sensitive to the varied opinions of other group members and how giving might impact their reputation (Haley & Fessler, 2005; List, 2006). To establish if group size has an effect independently of a “don’t give” social norm I would need to run an analysis comparing the donation rate in groups where an individual has chosen to give and where no-one has. Unfortunately, given the low rate of donation, my available data on co-giving is very scant and answers provided by running this analysis would be meaningless. A much more simple explanation could be that individuals on their own were simply less distracted and were more likely to notice the mendicant.

The density result, which shows that people are less likely to give when there are more people around (Figure 3.2, Table 3.2), is consistent with several experimental results showing that in larger groups it is harder to maintain cooperation in public goods dilemmas (Capraro and Barcelo, 2015; Duffy and Xie, 2016). I have been considering the interaction between a focal and a pedestrian as being the theoretical equivalent of a dictator game. However, when the interactions of everyone on a piece of pavement at the same time are considered it could be thought of as public goods game. The individual donor paying a cost with a “benefit” of assuaging the guilt of all pedestrians. Other explanations could include that when there are a larger number of people present there is diffusion of responsibility, as in the bystander effect (Darley and Latane, 1968). Alternatively, the social norm effect as described above could also apply here, a larger number of people choosing not to give indicating that there is a norm of “don’t give” leading to lower levels of giving (Jones and Linardi, 2012).

My next hypothesis addresses how the type of focal I observed influences pedestrians’ rate of donation. I hypothesised that when there was a higher level of reciprocity in the interaction that there would be a higher rate of donation. This hypothesis was borne out in the case of buskers who did indeed receive a higher level of donation than mendicants (Figure 3.3, Table 3.3). Although other explanations could include that pedestrians may have noticed buskers more than mendicants due to fact that they are busking. Charitable collectors, however, received more than either mendicants or buskers (Figure 3.3, Table 3.3). There could be several possible explanations for this. It could be the case that mendicants and buskers are both private individuals whilst charities are publicly recognised institutions that are required to abide by legal frameworks. Therefore, when donors give money to a charitable collector they may feel that this donation is a more trusted way of “doing good” than giving to a private individual. If a pedestrian's main motivation for giving is not reciprocal but to enhance their reputation or to make themselves "feel good" as a proximate mechanism, then there are also fewer social injunctions against giving to charitable collectors than to either mendicants or buskers (Phelan *et al.*, 1997).

Social contagion did not influence giving behaviour in this study, contrary to my hypothesis (Figure 3.5, Figure 3.6). In the temporal model (Table 3.2), whilst the density of pedestrians did influence donation behaviour, the number of donations that occurred in the two minutes prior to donation did not have a significant effect on current donation behaviour. This suggests individuals are not copying behaviour that they have just seen. Similarly, I also carried out an experiment to test this effect where I made experimental donations to focal mendicants. In this experiment, there were no differences seen in the rate of donation observed before and after experimental donations. There are several possible explanations for this. Social contagion has been most consistently observed in systems where individuals already know one another socially (Fowler and Christakis, 2010). Where social contagion has been documented in naturalistic systems with strangers this has tended to be in contexts like road crossing where the focal behaviour takes place simultaneously (Faria, Krause and Krause, 2010). Overall, this study did not support social contagion as a mechanism for explaining donation behaviour on the street.

I found that women did tend to give more frequently than men (Figure 3.4, Table 3.4). However, this result was complex and I found that the context in which individuals gave influenced their behaviour. Men giving significantly less often to charitable collectors drove this overall result, with no significant differences between the sexes for mendicants and buskers (Figure 3.4, Table 3.4). There could be several explanations for this. It has been discussed in the charitable giving literature that men and women give to charity for subtly different reasons and that they give to different charities, with women being far more likely to give to human charities that deal with homelessness and healthcare than men (Dufwenberg and Muren, 2006; Einolf, 2011; Chowdhury, Jeon and Saha, 2017). As discussed earlier in this chapter donation towards buskers might not be considered as a charitable donation before which might explain the lack of a significant difference between men and womens' behaviour in this instance (Table 3.4). What is harder to explain is the lack of difference in giving to mendicants. It has been suggested that women tend to give to charities as they feel a sense of responsibility whilst men are more likely to give when they feel they can gain reputational benefits (Willer, Wimer and Owens, 2015). This analysis was concerned only with singletons and spur of the moment decisions and it could be the case that neither feelings of responsibility or reputational concerns were driving pedestrians decision-making in this instance, which may explain the lack of difference in this result. Earlier parts of this chapter also indicate that pedestrians did not increase their donation behaviour in the presence of larger group, suggesting that individuals may not be using this behaviour as a form of reputational prosocial signalling. Furthermore when I used a similar model to address the interaction between the sex of the donor and the sex of the recipient (Table 3.5), the interaction was not significant. However, it did approach significance suggesting that males may give qualitatively less to men than women. In order to establish this effect clearly further experiments would be required.

Overall, this chapter has demonstrated that individuals walking along a street are influenced by social factors when they are making decisions about how to cooperate with individuals asking for money. Group size and density both play an important role. Individuals on their own are more likely to give, as well as individuals experiencing a lower level of foot traffic. Pedestrians do not seem to be affected by social-contagion when making decisions about cooperating in the street. The type of individual asking them for money is important, with charitable collectors the most likely to receive donations followed by buskers and mendicants, suggesting that the reciprocal nature of relationships may play a role in decision-making. Finally, the sex of the donor is important with women being more likely to give to charitable collectors than men, however, this result is not consistent across mendicants and buskers.

Chapter 4 – The dog poo dilemma: social influences on cooperation in a wild public goods game

4.1 Introduction

A major challenge in understanding the evolution of stable cooperation between non-relatives is in identifying the mechanisms that maintain public goods (Boyd & Richerson, 2009; Fehr & Fischbacher, 2003). Public goods problems develop wherever the benefits of a social action accrue to all members of a group, while any costs are borne by individual actors (Hardin, 1968). They are vulnerable to invasion because cheats can take advantage of other individuals leading to the breakdown of cooperation (El Mouden, West and Gardner, 2010). Public goods are a common feature of social systems, from microbes to primates (Kahneman and Knetsch, 1992; Heilmann, Krishna and Kerr, 2015). These problems are ubiquitous in human society and underlie a significant proportion of the problems that society faces, ranging from international cooperation to reduce carbon emissions through to regulation of employment and financial services (Milinski *et al.*, 2006). For example, no one country will undertake to invest heavily in carbon capture technology unless there are assurances that others will provide a market for that technology. Understanding how cooperation is maintained in these situations and how dilemmas can be resolved is therefore of potentially global importance.

Public goods problems can be resolved by deploying mechanisms that reduce the payoffs of cheating, such that cheating no longer pays (‘conditional cooperation’, including punishment and forms of reciprocity (Fischbacher, Gächter and Fehr, 2001; Nowak and Sigmund, 2005). They can also be resolved by reducing the extent to which resources are publicly accessible, which in humans is often achieved through private ownership (Ostrom, 2003). Private ownership of resource can stabilise public goods dilemmas as owners who have a future interest in a particular resource have a greater incentive to invest in its maintenance (Hardin, 1968).

Experimental investigations of human public goods games have tended to confirm theoretical expectations, finding that cooperation declines over time except where participants receive some type of reputational information, or are able to punish selfish behaviour (Fu *et al.*, 2008; Gächter, Renner and Sefton, 2008). However, Most of this work has been carried out on experimental subjects in a laboratory (Levitt and List, 2007; Benz and Meier, 2008). Laboratory studies of cooperative behaviour have several key flaws: (i) people have an intense awareness of the fact that they are under observation; (ii) there is an experimental demand effect; (iii) there are no negative pay-offs in the laboratory and; (iv) there is a participant sampling bias (Haley and Fessler, 2005; Benz and Meier, 2008; Barmettler, Fehr and Zehnder, 2012; Burton-Chellew and West, 2013). Given these problems with laboratory games and the limited research on naturally occurring public goods, we require a wild system that will allow us to make more generalizable conclusions about human cooperative behaviour and the importance of different mechanisms in resolving these games (Yoeli *et al.*, 2013).

The ideal context for investigating the causes of variation in contributions to a naturally occurring PG is one which: (i) occurs in public space; (ii) is frequent; (iii) has clear costs to individual cooperators; (iv) has clear benefits to society as a whole; (v) occurs across a range of socio-demographic conditions; (vi) is easy to observe without detection; and (vii) is easy to manipulate without detection or ethical concerns. Despite the frequency of public goods games in human interactions, very few meet these criteria. In this study I frame the abandonment and retrieval of dog faeces in public parks as a public goods game, and use the rate of faecal abandonment as a measure of cooperative behaviour. In this framing, the cost of picking up dog faeces, in terms of time, disgust and risk to health (real or perceived) accrues to the individual, while the benefit of having a clean park accrues to all park users (Hardin, 1968; Kolodko, Read and Taj, 2016). This system allows me to manipulate the level of canine faeces in parks and so test whether people are monitoring and responding to others behaviour.

Although dog fouling may seem a trivial problem, it frequently ranks among the top 10 issues of citizens concern; it imposes substantial costs on municipal authorities (Keep Britain Tidy, 2014); and limits the appeal and accessibility of urban green spaces. Despite this, there has been limited formal investigation. A recent systematic review identified that there were no robust studies of interventions to combat dog fouling (Atenstaedt and Jones, 2011). The limited evidence available suggests that fouling is most likely to be committed by males, those with a lower income and those walking their dogs off the leash (Wells, 2006). These results are consistent with those from the, better developed, litter dropping literature, suggesting that younger individuals, those from lower socio-economic backgrounds and males tend to drop more litter (Krauss, Freedman and Whitcup, 1978; Durdan, Reeder and Hecht, 1985). Ease of disposal also makes a difference, with higher availability of bins leading to less littering (Finnie, 1973).

There are marked differences in the level of dog fouling in different places, ranging from 53.5% of dog walkers picking up faeces in parks Northern Ireland in 2006 to only 5% on Chicago sidewalks in 1980 (Jason, McCoy, Blanco, & Zolik, 1980; Wells, 2006). This suggests that local norms for abandoning or picking up faeces may apply. Whilst their rigour is in question, there have been several studies of interventions that appear to reduce fouling.

For example, in Chicago, community members were taught to pick up dog faeces using plastic bags and pooper scoopers, which in combination with a widely publicised legal ordinance for dog-walkers to carry pooper-scoopers, resulted in a sustained reduction in dog fouling two years after the intervention (Jason & Zolik, 1980). In the litter-dropping literature, community based approaches have also been found to be effective in causing sustained change (Wall *et al.*, 2009; Kolodko, Read and Taj, 2016). A more recent study by Keep Britain Tidy, although not peer reviewed, found that signs carrying images of eyes were effective at reducing dog fouling, indicating that individuals are sensitive to cues indicating that they are under observation (Keep Britain Tidy, 2014).

People can decide whether they will cooperate conditionally on the basis of information about others contributions (Fischbacher, Gächter and Fehr, 2001; Kocher *et al.*, 2008). The broken window theory suggests that people are more likely to behave in a non-cooperative way in places that are poorly maintained (Keizer, Lindenberg, & Steg, 2008). The original theory suggested that there would be higher levels of crime in areas with more broken windows, graffiti and abandoned cars (Kelling and Wilson, 1982). Empirical support for this is mixed (Keizer, Lindenberg, & Steg, 2008). In general, at sites which have a higher level of disorder people are less likely to behave prosocially, for example, one-shot naturalistic experiments of littering behaviour tend to show that people are more likely to litter at sites that are already littered (Reiter and Samuel, 1980; Cialdini, Reno and Kallgren, 1990; Ramos and Torgler, 2012). One longer term study, which considers 'not littering' as a private contribution to a public good, found that when the level of cleaning by the local authority is reduced there is an increase in littering (Dur and Vollaard, 2014). This suggests that individuals are sensitive to investments made by others and will adjust their behaviour conditionally. If an individual perceives that someone else is investing less, their pay-off for cooperating is reduced leading to a lower level of cooperation. There are also studies which suggest that the broken window theory only holds for particular types of anti-social behaviour, like robbery (Harcourt, 1998; Sampson and Raudenbush, 1999). I manipulated the perceived level of dog fouling in public parks to ask the question: Will people change their behaviour as a result in a change in their perception of others contribution? I would predict that in areas with higher levels of dog fouling there will be increased additional fouling.

People are sensitive to social context when making decisions about whether or not to cooperate in social dilemmas and often react to information about how observable their behaviour is when making decisions (Rockenbach and Milinski, 2006; Yoeli *et al.*, 2013). In laboratory games, introducing reputational concerns, for example, allowing individuals to be identified across several games, can induce higher levels of cooperation. Theoretically people can seek to maximise their pay-offs by maintaining a cooperative reputation and so behave more cooperatively when they feel their behaviour is under scrutiny (Yoeli *et al.*, 2013). Similarly, in naturalistic field studies gentle proxies of observation can induce higher levels of cooperation (Milinski, Semmann and Krambeck, 2002; Rosenbloom, 2009; Wiessner, 2009). For example, numerous studies have demonstrated the effect of posters with eyes on them in inducing prosocial behaviour (Haley and Fessler, 2005). Such posters have been used to induce higher levels of donation in honesty boxes in academic tea rooms, deter bike thieves and discourage people from littering (Bateson, Nettle and Roberts, 2006; Nettle, Nott and Bateson, 2012; Keep Britain Tidy, 2014). In Chapters 2 and 4 I also demonstrated that people are sensitive to the number of other pedestrians when making cooperative decisions. Therefore, I expect that the risk of being observed will affect dog fouling, with less fouling where parks more overlooked.

In my study I was interested in the way in which affluence affects willingness to invest in public goods as socio-demographic factors frequently affect decision making in cooperative scenarios (Lamba and Mace, 2011). Whilst this has not been consistently demonstrated in the laboratory, most studies show that affluent areas display more cooperative behaviour whilst less affluent areas are less cooperative (Gächter, Herrmann, & Thöni, 2004; Holland et al., 2012; Wilson et al., 2009). For example, in a study where stamped addressed letters were dropped in the street in different parts of Dublin, the only variable that predicted rate of return was how affluent the area was (Silva and Mace, 2014). This finding does not hold universally, with some studies showing the reverse. For example, people in higher value cars were less likely to stop at pedestrian crossings in California (Piff *et al.*, 2012). Not dissimilarly, some reports suggest that individuals with higher incomes are less likely to pay attention to others on the street (Dietze and Knowles, 2016). Overall, the majority of studies show that socio-demographic effects play a strong role with more affluent areas producing higher levels of cooperation and I expected to replicate this result and see lower levels of dog fouling in affluent areas.

A frequently cited method for resolving social dilemmas is private ownership of resources (Hardin, 1968; Ostrom, 2003). For example, in the fishing industry, long term quotas, where fishermen have an investment in the future of their fishing grounds, have been shown to be effective in encouraging individual fishermen to limit their discards so that stocks have an opportunity to recover (Grafton, 1996; Branch, Rutherford and Hilborn, 2006). Similarly, in laboratory games, where ownership of resources is not possible, cooperation in public goods games tends to disintegrate rapidly (Fehr and Gächter, 2002). People also tend to behave more cooperatively towards individuals who they have an expectation that they will meet again, for example in areas with higher levels of house ownership (Bó, 2005). This effect, known as the “shadow of the future”, has been found in other species with similar reciprocal cooperative dynamics, such as cleaner fish (Oates, Manica and Bshary, 2010). In this study I expected that in areas with higher levels of home ownership fouling would happen at a lower rate.

Here I investigate whether social variables affect dog fouling rates in public parks: Specifically, I ask whether fouling is affected by: (i) the risk of being observed cheating; (ii) affluence; (iii) property ownership (independently of affluence); and (iv) measures commonly thought to correlate with civic-mindedness and contribution to public goods. I then conduct a large scale manipulation experiment to ask whether fouling rates are influenced by an apparent shift in the local norm.

4.2 Methods

4.2.1 Park Selection

To capture representative variability in socio-demographic factors, I used data from the Scottish Neighbourhood Statistics website to select 32 parks from the 147 parks listed on the City of Edinburgh Council website (Government, 2015). I ranked parks by percentage house ownership in the surrounding postcode in 2011, and, because the data were not positively or negatively skewed, selected 8 parks randomly from each quartile.

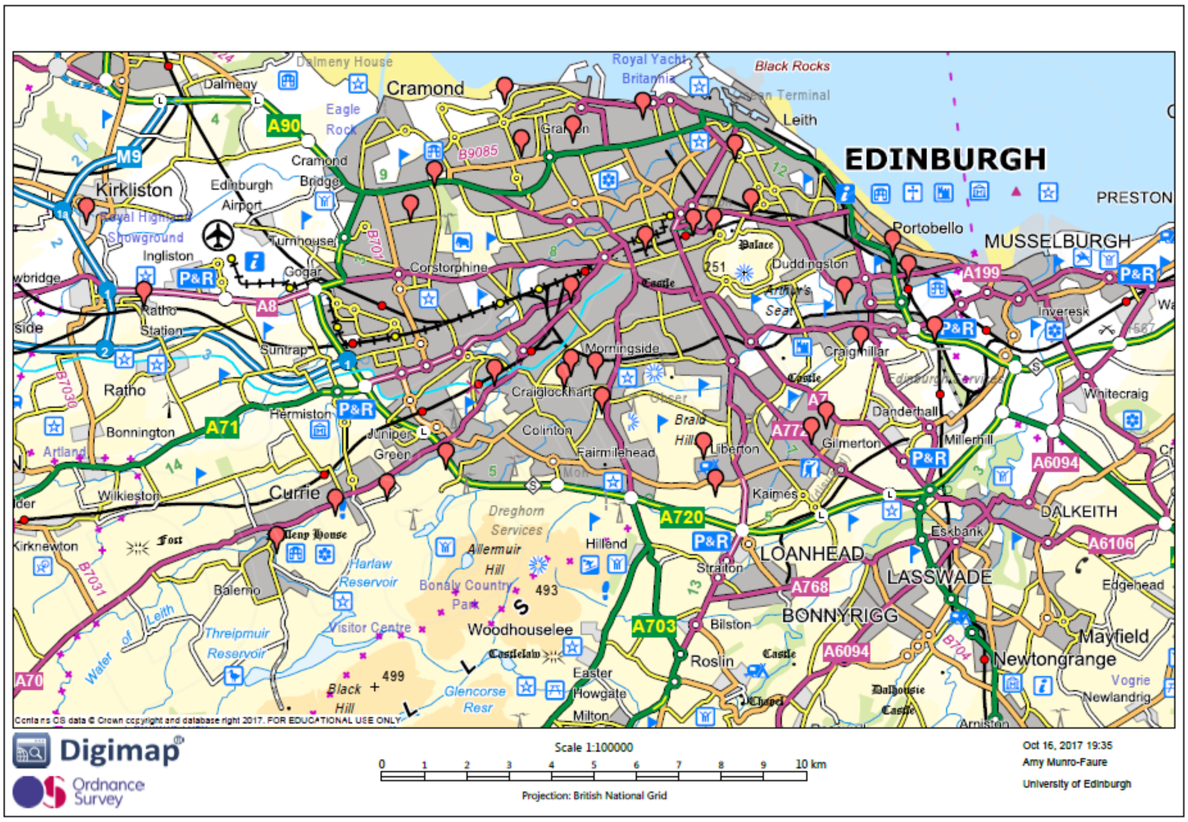

Figure 4.2 Park Locations

4.2.2 Pilot data and optimisation

Between 7th July 2015 and 7th August 2015, I conducted a pilot study to: (i) determine whether parks were appropriate for inclusion; (ii) identify the appropriate sampling method; (iii) identify a survey area in each park; (iv) estimate how many parks I could reliably visit and survey per day; (v) assess whether it was possible to measure the accumulation rate of new faeces, and (vi) optimise sampling frequency. I surveyed each park weekly, recording the location and total number of faecal deposits in each park.

Parks were discarded if they: (i) were being developed for housing; (ii) were inaccessible (due to fences or building work); (iii) were smaller than the standard survey area. When I rejected a park I randomly selected another park from the same quartile, until eight appropriate parks in each quartile had been identified.

I chose quadrat sampling over transect sampling because faecal deposits were usually widely distributed, at relatively low densities. I chose 50x50m squares as the standard sampling plot, being large enough to capture variation in dog faeces between parks but small enough to be feasibly surveyed weekly. I surveyed each quadrat by walking the length of the quadrat at five metre intervals and recording the number of faecal deposits in each 5x5 m square.

Within each park, I located the base of the quadrat on a main path leading to the park gate, and placed them such that they consisted of ≥80% open lawn, were reasonably flat and contained no major topographical features. All quadrats were measured using a measuring tape and mapped using a standardised grid (Figure 4.3). The corners and 10m intervals were marked out using brown spray paint on the ground to ensure consistency of sampling between weeks. Salient features were mapped (such as trees, flower beds, benches, basketball nets). In two very narrow parks, a 100 x 25m quadrat was used instead of the standard 50 x 50 m quadrat. The GPS co-ordinates of the corners of the quadrats were recorded.

This pilot revealed that summer fieldwork was inappropriate because rapid grass growth concealed faeces, and frequent mowing by the local authority destroyed them.

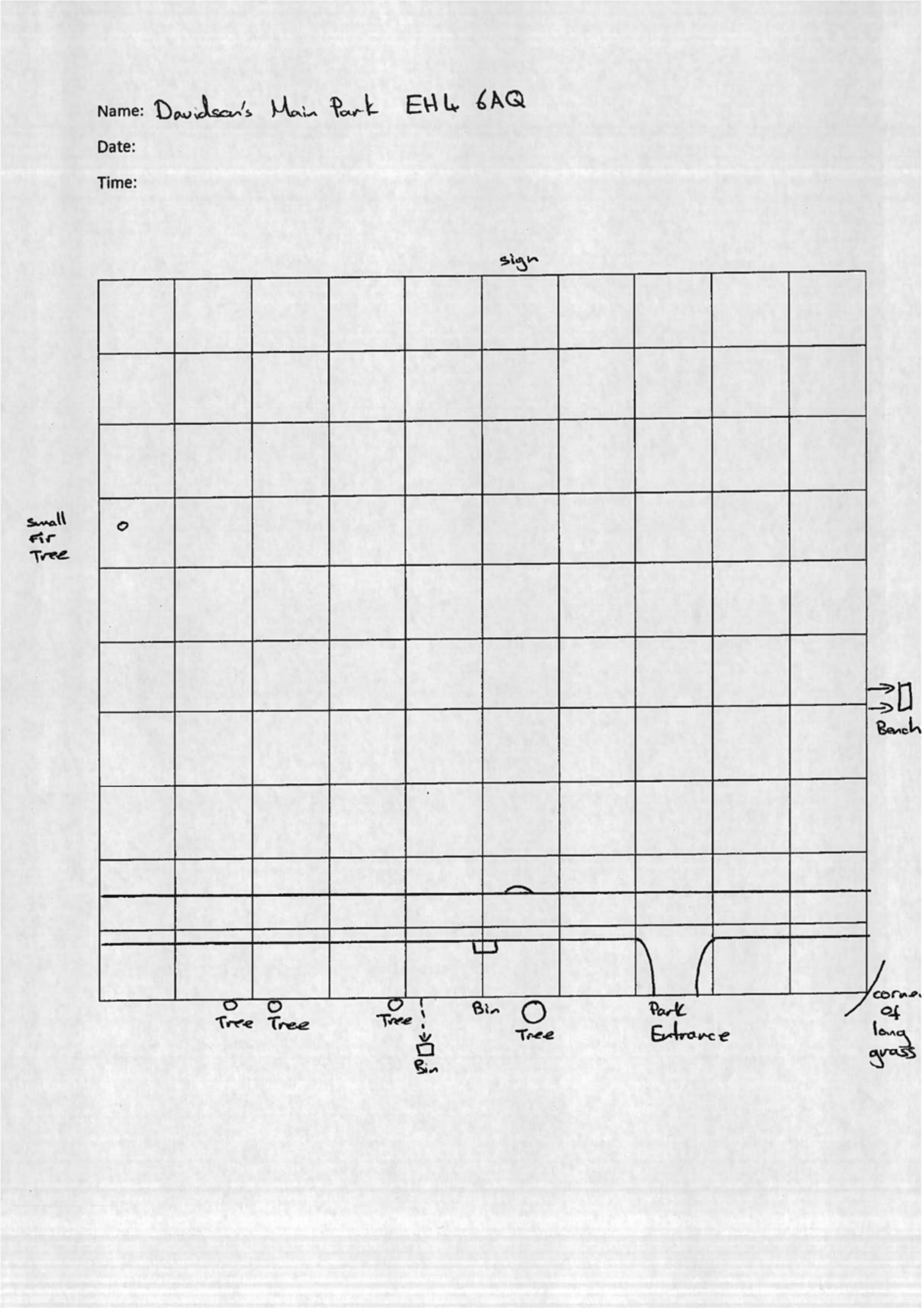

Figure 4.3 Example of a sample area map

4.2.3 Determining Park Usage

In order to plan sampling frequency during experiments, I used a pilot study to determine the consistency of park usage. I undertook two types of observation in each of a selection of pilot parks. Firstly, I carried out an all-day observation in eight of the parks (Leith Links, Morningside Park, Calton Hill, Allison Park, Newcraighall Park, Bingham Park, Mortonhall Community Park and Clermiston Park), from 06:00 to 21:00. I recorded the number of park users, dog walkers and dogs per hour. Secondly, I carried out hour-long observations in six parks (Newcraighall Park, Abercorn Park, Leith Links, East Pilton Park, Davidson's Mains Park and Murieston Park) every day at the same times (10:30 – 11:30, 11:50 – 12:50, 13:15 – 14:15, 14:45 – 15:45. 15:55 – 16:55, and 17:10 – 18:10). I calculated intra-class correlation using the psych package in R on both hour by hour and day by day data.

This analysis determined that surveying each park for one hour, once a week provided an accurate estimate of park usage (Table 4.1, Table 4.2).

4.2.4 Intra-Class Correlations

Table 4.1 Intra-Class Correlations by Day

Measure	Park Users	Dog Walkers	Dogs	Total People
Subjects	6	5	5	5
Raters	5	5	5	5
ICC Agreement	0.8228507	0.680289	0.792522	0.8231484

Table 4.2 Intra-Class Correlations by Hour

Measure	Park Users	Dog Walkers	Dogs	Total People
Subjects	8	8	8	8
Raters	15	15	15	15
ICC Agreement	0.5953515	0.7504787	0.7600457	0.6221941

4.2.5 Do human social variables affect dog fouling rates?

To investigate whether variation in dog fouling correlates with human variables, I carried out weekly observations for four weeks 01/02/2016 to 26/02/2016. Each week, I recorded the total number and location of all faeces in the experimental quadrats. From week 3 I recorded the number of new faecal deposits since the previous week, and the number of old faecal deposits that remained and had not disintegrated or been removed. My research assistant also conducted an hour long usage survey each week, recording the number of human park users, number of dog walkers, number of dogs. To correct for park size, we measured park area in m^2 using the Ordnance Survey ‘digimaps’ online service.

To test whether the risk of being observed affected fouling rates, we calculated ‘overlookedness’ for each park, measured as the number of windows visible from the centre of the quadrat. To test the effect of affluence, we obtained mean 2013 house price for the adjacent post code from Scottish Neighbourhood Statistics (Government, 2015) To test the effect of property ownership, we obtained percentage house ownership in the adjacent post code from Scottish Neighbourhood Statistics 2011 (Government, 2015).

To analyse the social variables that had an explicit *a priori* prediction (those listed above), I constructed a series of generalised linear mixed models (GLMMs) with a negative binomial error structure, total number of faeces recorded per survey as the response variable, and Park as a random term (data had an over-dispersed Poisson structure). I tested each variable in turn by fitting it as a single fixed effect.

In addition to the explanatory variables tested above in isolation, I examined the literature to generate a list of additional variables that might influence fouling rates. These were:

(i) The effect of the possible presence of small children (which may either correlate with greater vigilance by bystanders or heightened feelings of responsibility). Measured as distance to nearest primary school using google maps.

(ii) Two variables commonly assumed to indicate greater civic-mindedness: the percentage of left leaning voters at the ward level, and percentage voter turnout at the ward level. These were obtained for the 2017 Scottish Local Elections (Scottish Parliament, 2017).

(iii) To rule out possible effects of direct enforcement, the distance to nearest Police station, using google maps.

I then constructed a multivariate negative binomial GLMM with total faeces counted per week as the response variable. I constructed a maximal model containing both the variables with explicit predictions and all the additional social variables as fixed effects. To select the candidate fixed effects, I first determined whether any of these measures were correlated by building a correlation matrix (Table 4.8). I then conducted a principal component analysis (PCA) to identify any potential composite axes (such as "park use" or "socio-demographics") for use as predictors in the GLMM.

The correlation matrix revealed a strong correlation between number of dog walkers and number of dogs (Table 4.8), so I used number of dogs alone in all subsequent models. The PCA did not detect any components that explained significant variation (Proportion of Variance explained by Component 1 = 0.282), so no composite axes were used in subsequent models. To account for overdispersion, I used an observational level random effect with a Poisson distribution, and compared the output with a standard negative binomial model (which generated qualitatively similar results).

To generate a minimal model, I performed a reverse stepwise deletion based on p-values and used likelihood ratio testing to establish that each model was not significantly different to the previous iteration of the model. I used this methodology rather than AIC values as the results were qualitatively similar, all steps resulted in lower AIC values. Some statisticians would also argue that performing a stepwise regression with p values is essentially the same as performing one with AIC, as they are in many ways restatements of the same thing (Harrell, 2001). Both methodologies are also subject to similar methodological pitfalls as they involve testing repeated measures.

All analyses were carried out in the lme4 package of R version 3.3.1, RStudio 1.0.136.

4.2.6 Are fouling rates sensitive to an apparent shift in the local norm?

From week 5, parks were randomly assigned to one of three treatments which were distributed evenly across the quartiles: removal (n=11), addition (n=11) or control (n=10) (Table 4.3, Appendix 5). In each park I created a treatment area, around the path that ran along the base of each quadrat, (see Figure 4.4 for an example). The treatment areas were all 5 x 50m areas, except in the two parks with rectangular quadrats, where they were 2.5 x 100m areas.

In the removal treatment, I removed all dog faeces from the treatment area using a trowel and faeces disposal bags. The treatment was repeated each week for the remainder of the experimental period. In the addition treatment, the number of dog faeces was doubled from the mean baseline number recorded in the treatment area during weeks 1-4. Doubling was repeated for two weeks, and then maintained at the week 6 treated level for the remainder of the experimental period "addition" was achieved using artificial dog faeces (see below). In the control treatment no changes were made. During weeks 5, 6, 7, 8 and 9 I recorded the number of new faeces abandoned in the quadrat.

To analyse the experiment, I constructed a negative binomial GLMM, with new real faeces accumulated per week as the response variable, and Park as a random term. I fitted treatment (Control, Addition, Removal), week since start of treatment, and treatment*week as fixed effects. To account for overdispersion, I initially used an observational level random effect with a Poisson distribution. However, I dropped this after likelihood ratio comparisons of the final model with and without this term revealed no significant difference.

Treatment Type	**Before Treatment**	**During Treatment**
Control (10 parks)	Baseline faeces monitoring	Baseline faeces monitoring.
Addition (11 parks)	Baseline faeces monitoring	Amount of faeces doubled from mean baseline level in treatment area.
Removal (11 parks)	Baseline faeces monitoring	All faeces in treatment area removed.

Table 4.3 Treatment schematic

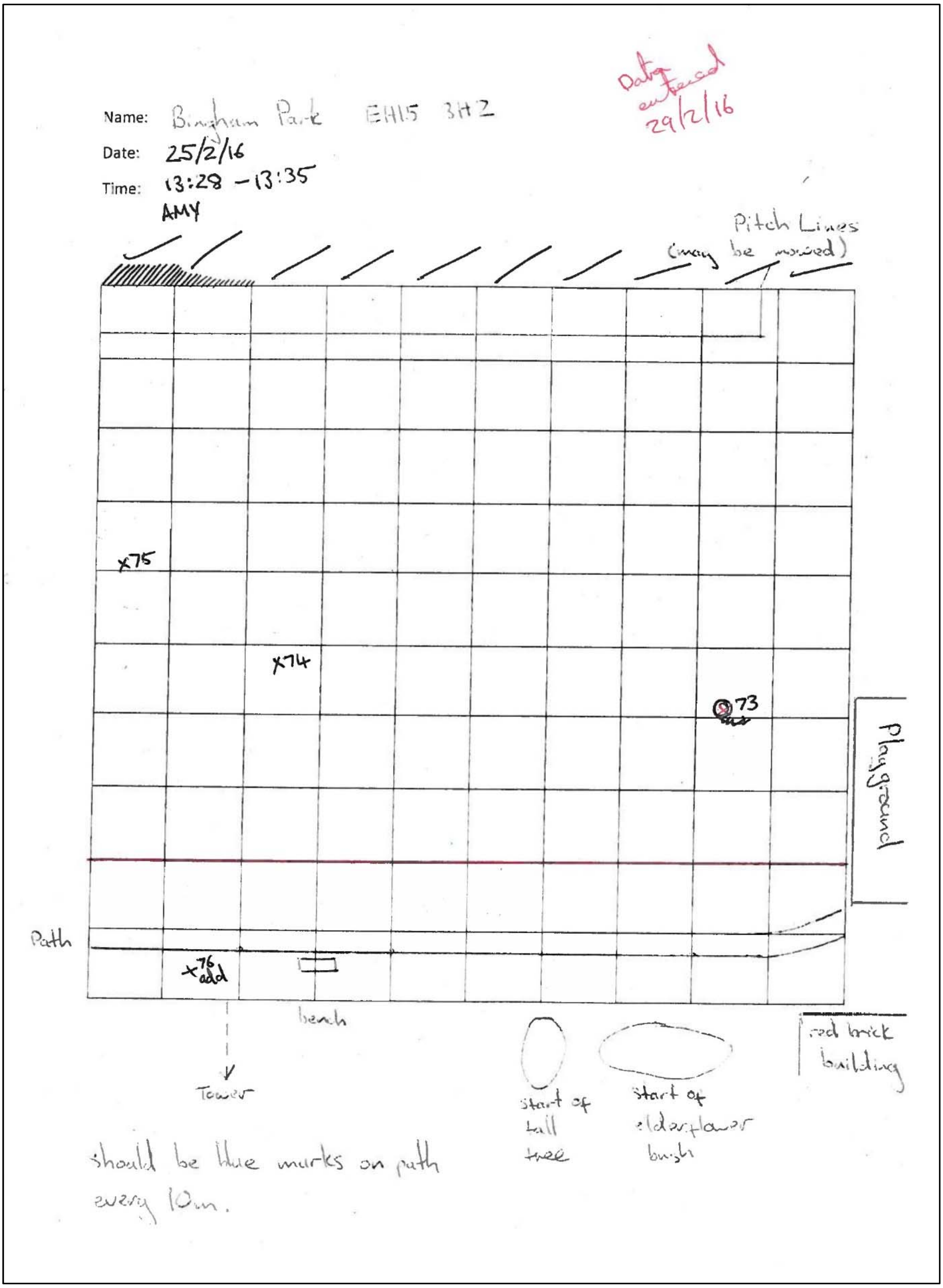

Figure 4.4 Example map showing treatment area and observations of faecal deposits. Black crosses represent new faecal deposits. A red cross circled in black represents an old deposit

that is still present. A black cross with the word "add" next to it shows where an experimental fake faecal deposit was added. The red line represents the boundary of the treatment area surrounding the path.

4.2.7 Making Artificial Dog Faeces

Commercially available plastic dog faeces were costly, unconvincing, could be construed as litter and may be toxic if eaten by dogs. Therefore I manufactured 200 artificial, hand-made faeces using a mix of porridge oats and watered down PVA glue (see Figure 4.4). Aliquots of the oat glue mix were rolled into sausages of varying thicknesses, which were then formed into faecal shapes and sprayed with 'mushroom' coloured paint. These models had a heterogeneous texture and I could easily vary the size of faeces to reflect natural variation in size and shape. Porridge oats and PVA are not toxic to dogs. During model development, I trialled a number of prototypes, and used a group of 20 peers to test realism (see Appendix 6 for the other prototypes tested and manufacturing process).

Figure 4.5 Example of Artificial Dog Faeces

4.2.8 Ethics Statement

All observational and experimental protocols were reviewed and approved by the School of Biological Sciences ethical review committee.

4.3 Results

4.3.1 Are dog fouling rates the product of number of dogs visiting a park?

There was no relationship between the number of dogs and the number of faecal deposits, see Appendix 7 for means and standard errors (Table 4.4, Figure 4.5).

Table 4.4 GLMM of the relationship between number of dogs visiting a park and fouling rates (n=128 observations from 32 parks over 4 weeks)

Fixed Effect	Event ratio (95% CI)	z Statistic	P(z)
(Intercept)	6.972 (4.470, 10.675)	9.020	$<2x10^{-16}$
Dogs	0.995 (0.972, 1.019)	-0.422	0.673
Random Effect	**Variance**	**Standard Deviation**	
Park	1.29	1.136	

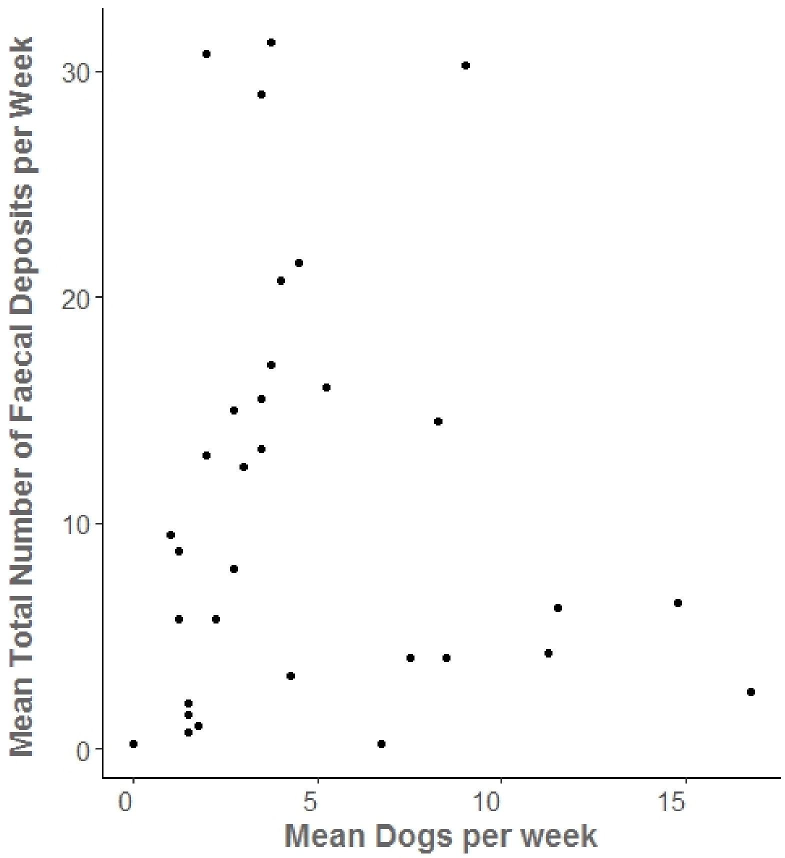

Figure 4.5 The relationship between number of dogs visiting a park and the number of faeces (data are means per Park).

4.3.2 Do human social variables affect dog fouling rates?

4.3.2.1 Does the risk of being observed affect fouling rates?

There was no relationship between overlookedness and number of faecal deposits (Table 4.5, Figure 4.6).

Table 4.5 GLMM of the relationship between overlookedness and fouling rates (n=128 observations from 32 parks over 4 weeks)

Fixed Effect	**Event ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	6.851 (4.498, 10.370)	9.402	$<2x10^{-16}$
Overlookedness	1.391 (0.921, 2.11)	1.609	0.108
Random Effect	**Variance**	**Standard Deviation**	
Park	1.223	1.106	

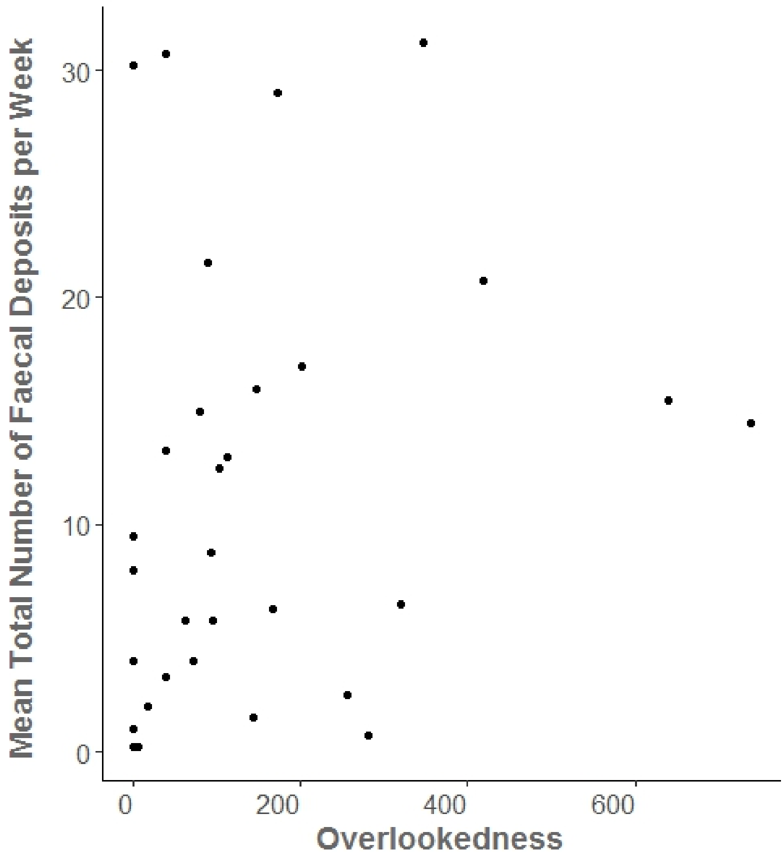

Figure 4.6 The relationship between overlookedness (number of windows visible from centre of experimental quadrat) and number of faeces (data are means per Park)

We further investigated the risk of being observed by examining whether the number of non-dog walkers using a park affected fouling rates, finding no effect (GLMM, event ratio 1.005 (95% CI -0.01-0.022, z=0.71, p=0.48). Moreover, there was no evidence of social policing among dog walkers themselves, with no effect of number of dog walkers on fouling rates (GLMM, event ratio 0.995 (95% CI 0.97-1.02, z=-0.37, p=0.71).

4.3.2.2 Does affluence affect fouling rates?

There was no relationship between average 2013 house price in the adjacent post code and fouling rates (Table 4.6, Figure 4.7).

Table 4.2 GLMM of the relationship between average 2013 house price in the adjacent post code and fouling rate (n=128 observations from 32 parks over 4 weeks)

Fixed Effect	**Event ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	6.896 (4.476, 10.409)	9.302	$<2\times10^{-16}$
Scaled House Price	0.748 (0.489, 1.135)	-1.401	0.161
Random Effect	**Variance**	**Standard Deviation**	
Park	1.251	1.118	

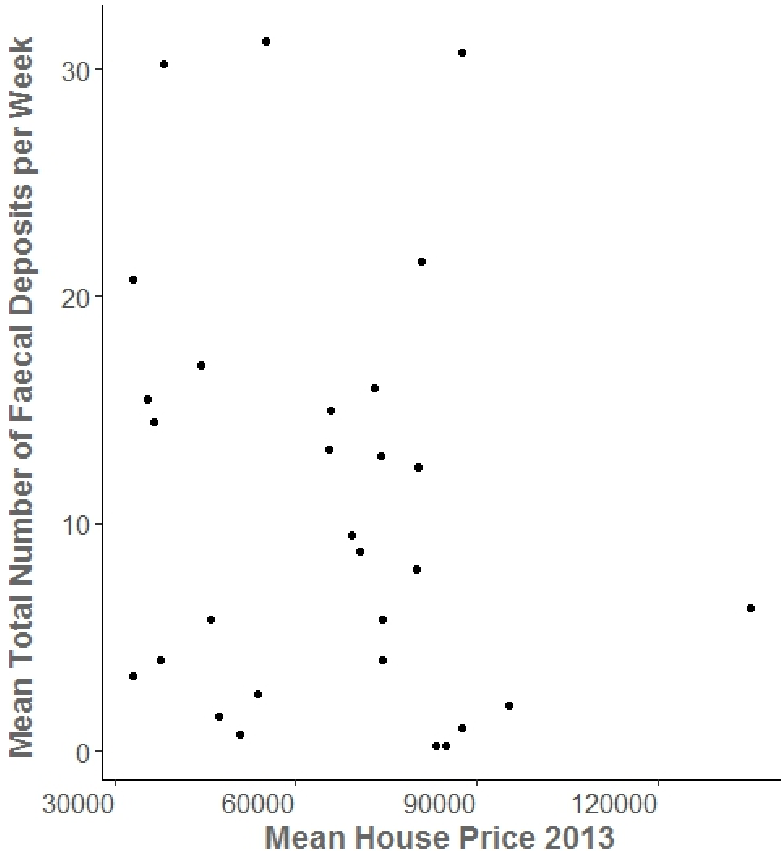

Figure 4.7 The relationship between average 2013 house price in the adjacent post code and number of faeces (data are means per Park)

4.3.2.3 Does property ownership affect fouling rates?

There was no relationship between percentage house ownership in the adjacent post code and fouling rates (Table 4.7, Figure 4.8).

Table 4.7 GLMM of the relationship between percentage house ownership in the adjacent post code and fouling rate (n=128 observations from 32 parks over 4 weeks)

Fixed Effect	**Event ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	11.640 (3.133, 42.582)	3.829	0.000129
% House Ownership	0.991 (0.972, 1.011)	-0.861	0.389290
Random Effect	**Variance**	**Standard Deviation**	
Park	1.295	1.138	

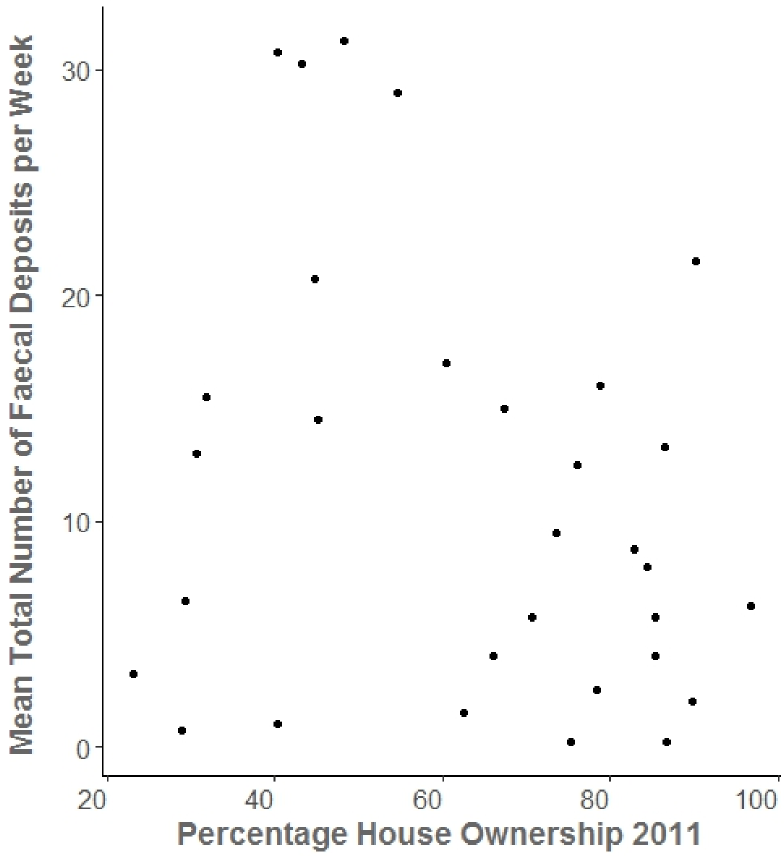

Figure 4.8 The relationship between percentage house ownership in the adjacent post code and number of faeces (data are means per Park).

4.3.3 Multivariate Analysis: do any social variables act together?

The maximal model detected no significant effect of any of the social variables on dog fouling rates (Table 4.9). After model reduction, distance to nearest primary school remained as the sole significant term (Table 4.10). Parks that were closer to a primary schools had more fouling (Figure 4.9).

Table 4.8 Correlation Matrix for Multivariate Analysis

	% Left Wing	% Turn Out	Park Size	Police Station	Primary school	House Price	% Ownership	Windows	People	Hectarage	Density	Park Users	Dog Walkers	Dogs
% Left Wing	1	-0.27	0.17	0.08	-0.49	-0.36	-0.26	0.35	0.19	-0.09	0.16	0.05	0.32	0.34
% Turn Out	-0.27	1	0.07	0.49	0.16	0.48	0.61	-0.43	-0.2	0.19	-0.36	-0.19	0.04	-0.01
Park Size	0.17	0.07	1	-0.15	-0.07	0.09	-0.23	0.12	0.36	0.06	-0.31	0.38	0.3	0.25
Police Station	0.08	0.49	-0.15	1	-0.17	-0.08	0.16	-0.26	-0.19	0.69	-0.26	-0.34	-0.31	-0.28
Primary school	-0.49	0.16	-0.07	-0.17	1	0.24	0.24	-0.38	-0.01	-0.14	-0.2	-0.2	-0.12	-0.1
House Price	-0.36	0.48	0.09	-0.08	0.24	1	0.6	-0.46	-0.23	0	-0.39	-0.08	-0.07	-0.15
% Ownership	-0.26	0.61	-0.23	0.16	0.24	0.6	1	-0.45	-0.16	0.04	-0.25	-0.33	0.13	0.08
Windows	0.35	-0.43	0.12	-0.26	-0.38	-0.46	-0.45	1	0.52	-0.15	0.58	0.36	0.23	0.22
People	0.19	-0.2	0.36	-0.19	-0.01	-0.23	-0.16	0.52	1	-0.06	-0.08	0.25	0.3	0.24
Hectarage	-0.09	0.19	0.06	0.69	-0.14	0	0.04	-0.15	-0.06	1	-0.36	-0.19	-0.26	-0.25
Density	0.16	-0.36	-0.31	-0.26	-0.2	-0.39	-0.25	0.58	-0.08	-0.36	1	0.08	0.12	0.14
Park Users	0.05	-0.19	0.38	-0.34	-0.2	-0.08	-0.33	0.36	0.25	-0.19	0.08	1	0.28	0.18
Dog Walkers	0.32	0.04	0.3	-0.31	-0.12	-0.07	0.13	0.23	0.3	-0.26	0.12	0.28	1	0.97
Dogs	0.34	-0.01	0.25	-0.28	-0.1	-0.15	0.08	0.22	0.24	-0.25	0.14	0.18	0.97	1

Table 4.9 Maximal GLMM investigating the social variables affecting rates of dog fouling in public parks (n=128 observations from 32 parks over 4 weeks)

Fixed Effect	**Event ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	6.713 (4.687, 9.616)	10.387	$< 2 \times 10^{-16}$
Park Users	1.056 (0.915, 1.218)	0.745	0.456
Dogs	0.960 (0.855, 1.078)	-0.690	0.490
% Left leaning voters	1.132 (0.683, 1.874)	0.482	0.630
% Turn out	1.166 (0.575, 2.361)	0.425	0.671
Park Size	0.842 (0.501, 1.414)	-0.650	0.515
Distance to Police Station	0.967 (0.428, 2.188)	-0.080	0.937
Distance to Primary School	0.688 (0.431, 1.100)	-1.560	0.119
House Price 2013	0.926 (0.528, 1.624)	-0.267	0.789
% Houses Owned 2011	0.916 (0.498, 1.684)	-0.283	0.777
Overlookedness	0.924 (0.444, 1.921)	-0.212	0.832
Number of people in area	1.302 (0.737, 2.299)	0.910	0.363
Hectarage	1.137 (0.605, 2.137)	0.399	0.690
Density	1.196 (0.640, 2.234)	0.560	0.575
Random Effect	**Variance**	**Standard Deviation**	
Observation Level Random Effect	0.02763	0.1662	
Park	0.99471	0.9974	

Table 4.10 Minimal GLMM investigating the social variables affecting rates of dog fouling in public parks (n=128 observations from 32 parks over 4 weeks)

Fixed Effect	**Event ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	6.712 (4.508, 9.806)	9.965	$< 2 \times 10^{16}$
Distance to Primary School	0.628 (0.421, 0.925)	-2.404	0.0162
Random Effect	**Variance**	**Standard Deviation**	
Observation Level Random Effect	0.02925	0.171	
Park	1.08788	1.043	

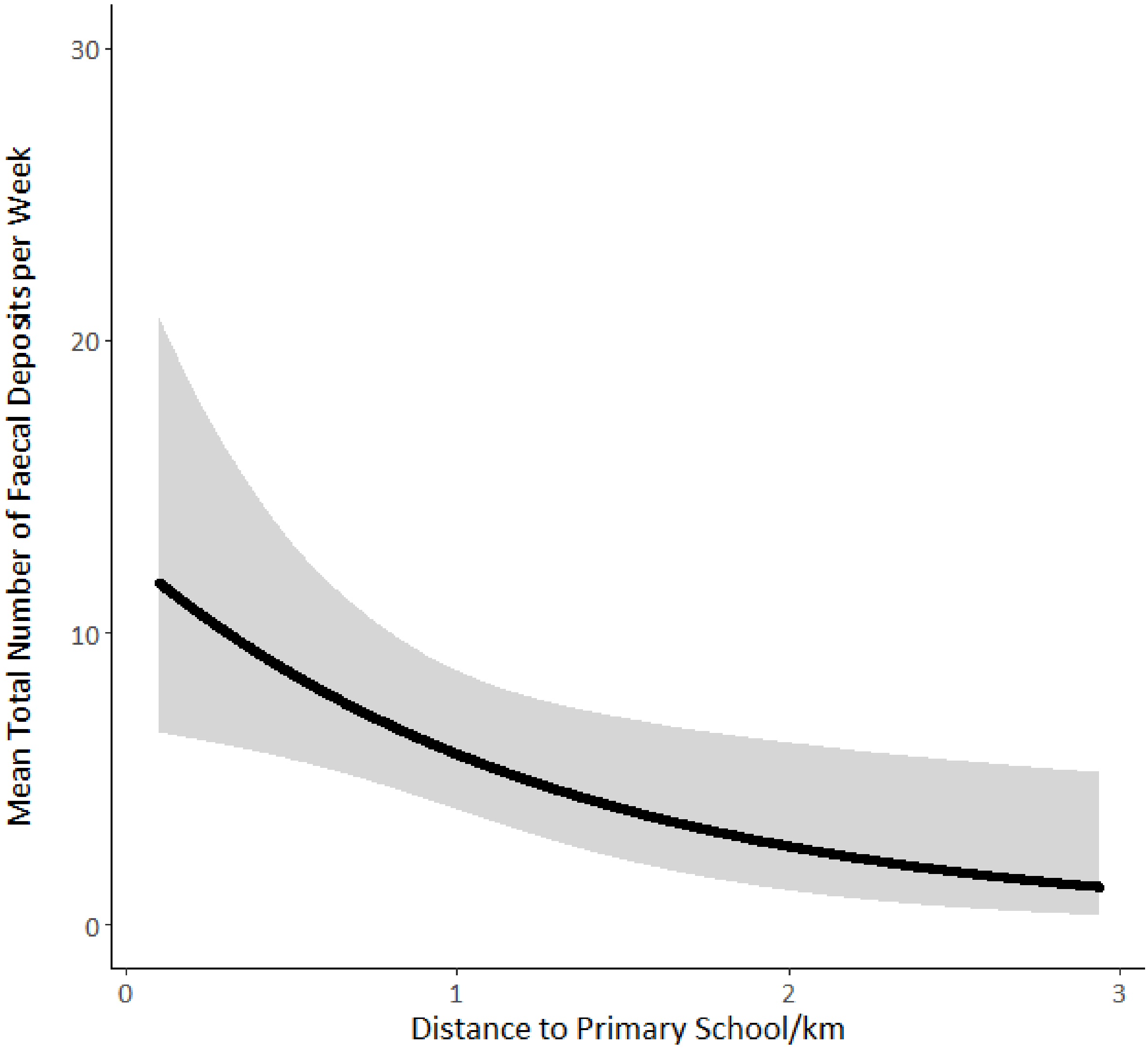

Figure 4.9 The relationship between distance to nearest primary school and total number of dog faeces (model predictions ±95% CI)

4.3.4 Are fouling rates sensitive to an apparent shift in the local norm?

Neither experimental addition nor removal of faeces had any detectable effect on the rate at which new faeces accumulated (Table 4.11, Fig 4.10), though there was a decline in accumulation rate over time (Table 4.11).

Table 4.11 GLMM examining the effect of Treatment and time on the accumulation rate of new faeces in public parks (n=160 observations, from 32 parks (11 Addition, 11 Removal, 10 Control), over 5 weeks).

Fixed Effect	**Event ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	3.745 (1.477, 8.942)	2.983	0.00285
Faeces Treatment	0.820 (0.273, 2.447)	-0.376	0.70729
Removal Treatment	1.196 (0.412, 3.600)	0.341	0.73297
Week Number	0.904 (0.848, 0.963)	-3.162	0.00157
Random Effect	**Variance**	**Standard Deviation**	
Park	1.295	1.138	

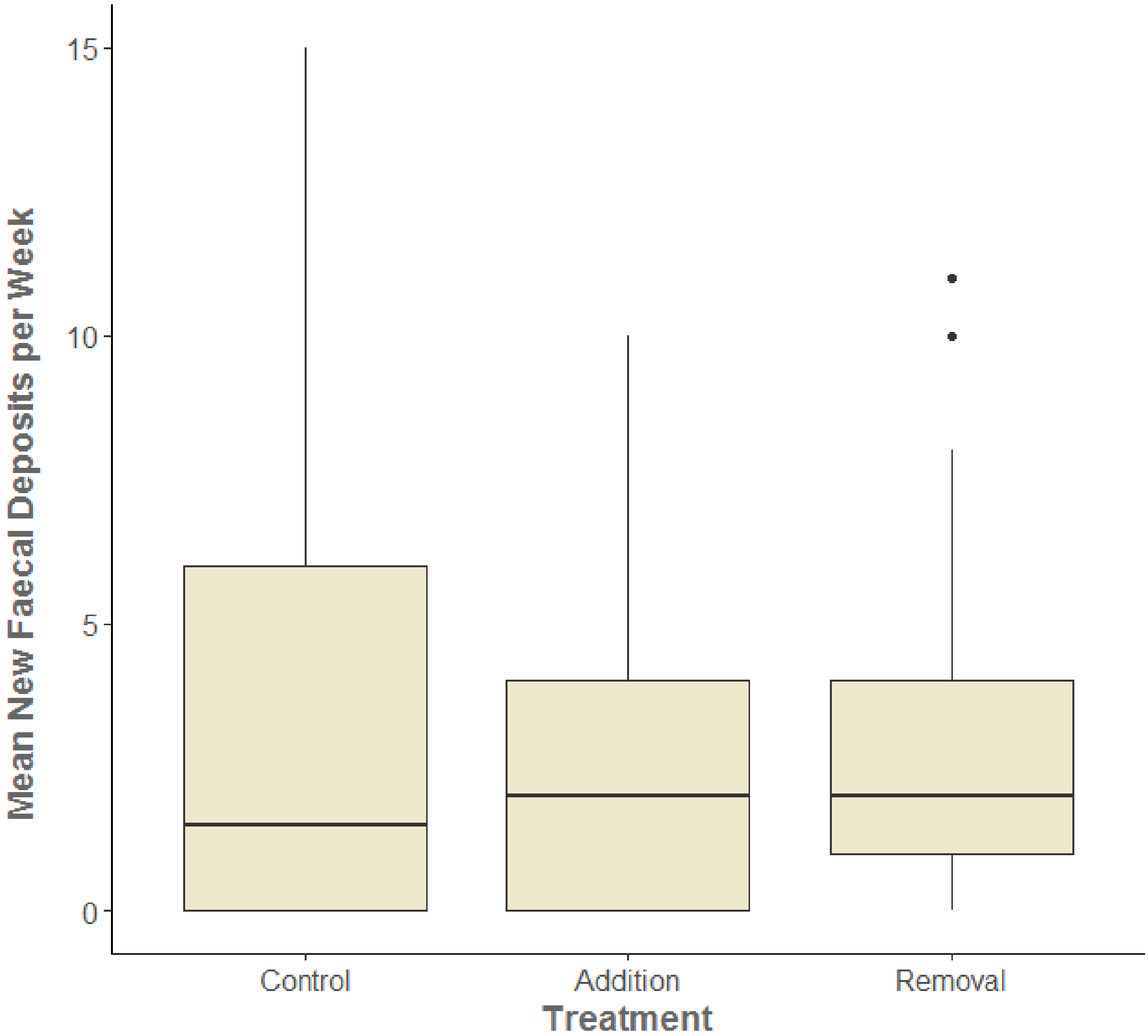

Figure 4.10 The effect of experimental manipulation on accumulation rate of new faeces (Control = 10 parks, Addition = parks, Removal = 11 parks, over 5 weeks)

4.4 Discussion

My main experimental result suggests that my treatments do not have an effect on park user and dog walker behaviour (Figure 4.10, Table 4.11). There could be several explanations for this. The first being the pragmatic one that perhaps the treatment was not strong enough for park users to notice the difference in faeces accumulation in the different experimental treatments. Baseline faeces accumulation was variable between parks meaning that my treatment of doubling the level of faeces or removing all faeces may have had substantially different effects in different parks. Although this was an explicit part of the experimental design to ensure that the treatments were sensitive to local norms, follow-up work could add a high level of faeces to parks uniformly, regardless of the baseline level and test the response to this.

A further explanation for the lack of an experimental result could be that this behaviour is highly norm based. The high level of variation between parks, where some parks have a high level of faeces and others have much less could be indicative of habitual behaviour. So the same individuals may have been using the park throughout the experimental period and did not change their behaviour in response to this treatment. It may take a substantially more explicit treatment, such as eye signs, a community intervention or stronger policing of anti-social behaviour or reward of prosocial behaviour to induce behaviour change (Jason et al., 1980; Keep Britain Tidy, 2014).

A further possibility could be that the two treatments may have worked in opposing ways. There is evidence that when common spaces are routinely cleaned individuals may free-ride on that regular cleaning to avoid doing it themselves (Dur and Vollaard, 2014). It may be that in the removal treatment individuals free-ride and do not pick up their dogs faeces. In the addition treatment, although I hypothesized that a higher level of faeces would lead to a higher level of abandonment of faeces by dog-walkers, as in the Broken Window Theory, dog faeces provokes a high level of disgust and it may be the case that this prompted more prosocial faeces-picking behaviour in these parks (Davey, 1994).

There is a significant effect of week number with the level of dog faeces declining across parks across the experimental period. There could be several explanations for this. The first being phenological, in that the experimental period ran from late February until the end of March. The temperatures were cold throughout this period and there was still snow on the ground in March which makes the loss of more faeces through washing away unlikely (Munro-Faure, personal observation). There was also an additional 2 hours and 29 minutes of daylight at the end of the experimental period in comparison to the beginning (Met Office, 2017). This additional daylight may have reduced the problem of normally prosocial dog walkers being unable to find their pets' faeces in the dark, leading to a reduction in faeces abandonment across the experimental period. Other explanations could include general stochastic variation in behaviour.

There is a final possibility that my survey method was insufficiently sensitive to pick up changes in level of dog faeces abandonment. I tested this possibility by running an analysis where I established that I could detect the treatments I put in place. I could detect this change. This in combination with the fact that I detect clear differences between parks suggests that my surveying method is sufficiently sensitive.

In terms of my observational work none of the factors I identified prior to data collection correlated with the level of dog faeces in public parks. There are several possible explanations for this. The fact that the level of dog faeces found does not correlate with the number of dogs being walked in the parks is suggestive of the fact that there is some type of social game being played (Figure 4.5, Table 4.4). Or, if not a social game, other extrinsic factors may be influencing people's decision-making behind abandoning faeces. In terms of socio-demography there is a high level of variation in both percentage house ownership and mean house price and it is fascinating that I have failed to replicate the patterns seen in other studies (Figure 4.7, Figure 4.7, Table 4.6, Table 4.7). Scotland has historically had a high level of social welfare provision in comparison to the sites of other dog fouling studies I have referred to (Jason et al., 1980; Mclean, Gallager, & Lodge, 2013; Wells, 2006). It could be the case that residents from a variety of socio-demographic backgrounds in Edinburgh have a fairly consistent view of how much they are willing to privately invest in a public goods game. It would be interesting to carry out further cross-cultural work to establish if this pattern holds in other locations.

In terms of overlookedness, it could be the case that people simply do not use windows as a cue of being observed (Figure 4.6, Table 4.5). There may be an evolutionary basis for this, some work suggests that we have an innate predisposition to be sensitive to human faces and that windows simply do not prime us to make social decisions (Dupierrix *et al.*, 2014; Wilkinson *et al.*, 2014). In chapter 4 I show that people are sensitive to the number of other pedestrians but not cars when making social decisions and it could be the case that this finding is indirectly being replicated here. Dog-walkers may actually need other people to be present in order to be primed to make a social decision as opposed to a rational one. There may be a confounding factor that in highly overlooked parks the population density is higher leading directly to a higher level of dog fouling. However, studies which investigate how population density impacts the length and frequency of dog walks do not support this. Dogs in more densely populated areas tend to get a higher frequency of shorter walks whilst dogs in lower density areas tend to get fewer longer walks, with the total amount of time dogs spend on walks being similar between areas with different population densities (Degeling, Burton and McCormack, 2012).

In terms of number of other park users, including dog walkers, the methodology I selected of using mean level of faeces in combination with mean park usage may not have been sufficiently sensitive to pick up an interaction between likelihood of being seen and dog fouling behaviour. None of the parks had a constant stream of usage throughout the observation sessions. It could be the case that even if there is a relationship between likelihood of being seen and dog fouling that even in parks with a high level of usage that there is a sufficiently high level of incidence of dog walkers being on their own in the park to not detect this relationship. Further work might involve extensive observation of parks from a discreet location to establish if actual faeces-picking behaviour varies with how many other individuals can see/be seen by a focal dog-walker.

Finally, there is a possibility that this could be a statistical artefact. I used a stratified random sampling approach to select my parks. This approach assumes that my response variable will be normally distributed, which it proved not to be. It could be the case that the high level of variation in faeces I see between parks, with some parks having consistently almost no faeces and other parks having high but variable levels of faeces, could mean that I was unfortunate in that my random approach selected “weird”, non-representative parks.

Given that none of my hypothesised explanatory variables correlated with the observed level of dog faeces in public parks I then decided to generate additional hypotheses to try and identify what other variables could explain these patterns in the hope of identifying hypotheses for future research. I initially tested these additional variables for correlation with one another and the pre-existing explanatory variables and discarded only one (Table 4.8). When I used a reverse stepwise deletion to remove all non-significant variables from a maximal model only one variable become significant: distance from a primary school (Figure 4.9, Table 4.9, Table 4.10). Parks that were closer to primary schools had more faeces than parks that were further away. There is a relatively simple explanation for this, which is one of convenience and minimal time and effort cost to the dog-walker. Dog-owning families may tend to walk their dogs at the same time as dropping off their children for primary school resulting in parks near primary schools receiving a higher level of faecal deposit than parks elsewhere.

I would like to highlight that this explanation is highly speculative and would require substantially more investigation to confirm. The finding that people are motivated by convenience, however, does correspond with some results from the litter-dropping literature (Finnie, 1973; Kolodko, Read and Taj, 2016). It would be interesting to carry out further work on how the availability of bins correlates with faeces abandonment, and perhaps carry out further manipulations where bins and perhaps stands containing faeces bags are added or removed from parks and the effect that this has on faeces abandonment. The hypothesis that people are more likely to carry out prosocial behaviour when it is convenient would also fit well within a public-goods framework as making something more convenient limits the cost of prosocial behaviour to the individual dog-walker.

Overall this study found that patterns in faeces abandonment in public parks are difficult to explain. The fact there is no correlation between the number of dogs using a park and the level of faeces suggests that there is some type of social game being played. However, what variables influence this game are not clear. There are tentative suggestions that convenience and limiting the cost of prosocial behaviour may be of importance to dog-walkers. Finally, it seems that dog-walkers are not sensitive to the higher or lower levels of disorder that I introduced in my experimental treatments when they are making decisions about whether or not to let their dogs foul.

Chapter 5 - Jumping the lights: Do people use social cues to make decisions about public goods dilemmas?

5.1 Introduction

In the previous chapter I examined how the usage and overlookedness of a public space influences decision-making in a naturally occurring public goods game and if individuals are sensitive to evidence that others have cheated. Here, I will ask how the density of observers influences individuals' decision-making in public goods games in real time and how individuals' decisions change when they observe others cooperating or cheating instead of just the evidence of cooperation or cheating.

Human cooperative behaviour between anonymous non-relatives presents a problem. This cooperation is relatively stable despite constant selection for cheating. The public goods game is a structure of pay-offs which underlies much human cooperative behaviour. Individuals invest in a resource at an individual level whilst benefits accrue to all members of a group, providing an incentive to cheat. These games have largely been studied using volunteers in laboratory models (Fehr and Gächter, 2002; Levitt and List, 2007). Cooperation usually declines over time in iterated public goods games (Fehr & Gächter, 2002; Levitt, & List, 2007). There are several mechanisms which resolve these dilemmas, including policing, punishment and reputational effects. These mechanisms prevent cooperation from disintegrating by changing the pay offs available to participants. A free-riding individual may receive a lower net pay-off if co-operators can identify and punish free-riders. Similarly reputational effects can mean that free-riders may miss out on the benefits of cooperative interactions in the future, if they do not keep up their side of the bargain (Boone and Buck, 2003; Gintis and Fehr, 2012).

Our current understanding of the generalizability of laboratory results is weak (Levitt *et al.*, 2007). For example, whilst there is debate about the role of cultural variation in laboratory experiments, there is a recruitment bias towards western undergraduates (Lamba and Mace, 2011). Laboratory games necessarily use simplified payoff structures, and assume that people's decisions reflect or even predict their decisions in natural conditions with similar payoff structures. However, these laboratory games assume that participants perceive losses from experimental endowments in the same way as they perceive real life costs. It is ethically unacceptable for participants to leave a laboratory study worse off than they arrived (British Psychological Society, 2014). However, real life cooperative decisions carry implications, positive and negative, which people can account for in their decision-making (Maxwell N Burton-Chellew and West, 2012; Winking and Mizer, 2013). People also tend to change their behaviour when they are aware that they are being observed (Haley and Fessler, 2005). Related to this is the experimental demand effect, where the behaviour of experimental participants is influenced by their perception of what researchers expect them to do (Barmettler, Fehr and Zehnder, 2012). This effect makes the implications of laboratory results unclear in a naturalistic setting.

Critiquing laboratory experiments by studying analogous natural systems is important if we want to understand the drivers of human cooperative behaviour (Winking and Mizer, 2013).The pay-off structure of public goods dilemmas is common in human society. Global, examples include financial markets and fishing quotas, however, there are also local examples that people experience daily. In order to understand what is driving cooperative behaviour and critically evaluate standard laboratory results we need to identify public good games that can be easily observed, quantified and manipulated. For example, cyclists on at pedestrian crossings. Cyclists stopping at red lights experience a small time cost and the energetic expense of regaining speed (Kautz and Neptune, 2002). All other road users benefit from cyclists following the rules as the roads are safer, but the benefit to the individual cyclist is minimal, creating an incentive for cyclists to jump lights.

There are laws clearly stating what behaviour is illegal , yet the probability of detection and sanctioning is low (Rehfisch, 2012, Appendix 8). Therefore, cyclists are engaged in a public goods game with clearly stated rules, but imperfect policing. A study carried out in Norway showing that both cyclists and motorists' behaviour aligned more closely with the game theoretic solution than traffic rules provides further evidence for this (Bjørnskau, 2015). Other studies of red light infringements by cyclists have not framed the interaction as a public goods game, and have focussed on intersections as opposed to pedestrian crossings, however, they have detected several trends: (i) Light jumpers are more likely to be young and male (Rosenbloom, 2009; Wu, Yao and Zhang, 2012; Johnson *et al.*, 2013). (ii) Cyclists are less likely to jump when there are pedestrians waiting at a crossing (Rosenbloom, 2009). (iii) Cyclists are more likely to jump if there is already another cyclist jumping (Johnson *et al.*, 2011). (iv) Jumping is less likely if cyclists knew they could be fined (Johnson *et al.*, 2013). (iv) Cyclists are more likely to jump if they were not wearing a helmet (this conflicts with work which suggests that helmet-wearers take more risks) (Pai and Jou, 2014; Gamble and Walker, 2016). In the absence of reliably imposed sanctions, it is likely that mechanisms like reputation effects and social contagion may mediate this behaviour.

Cooperative behaviour can be influenced by reputational effects. As discussed in earlier chapters, people can alter their cooperative behaviour when under observation. For example, the bystander effect is a well-documented phenomenon where individuals' behaviour will change depending on a particular threshold number of observers (Darley and Latane, 1968).

Similarly, in Chapter 3 of this thesis, I demonstrated a density effect where pedestrians were less likely to donate to mendicants when they were in a denser crowd. Even a gentle proxy of observation can cause changes in cooperative behaviour. The "seeing-eye" effect describes where a poster with eyes on it can prompt people to behave more cooperatively (Bateson, Nettle and Roberts, 2006). People make these changes as they care about their reputations, having a poor cooperative reputation may result in few opportunities to engage in reciprocal interactions in the future, leading to a lower net pay-off. In this study I will ask whether cyclists change their light jumping behaviour in response to how many people can see them. I predict that when behaviour is more observable fewer cyclists will jump the lights.

There is evidence that social contagion, where individuals copy behaviour from others, can play a role in cooperative behaviour (Kearns *et al.*, 2009; Christakis and Fowler, 2010; Tsvetkova and Macy, 2014). There are many studies suggesting that social contagion can influence behaviour, both prosocial (Van baaren *et al.*, 2004; Shang and Croson, 2009) and antisocial (Faria, Krause and Krause, 2010). Broadcasting of prosocial behaviour tends to result in observers engaging in more prosocial behaviour (Galaskiewicz and Burt, 1991; Fowler and Christakis, 2010; Lacetera, Macis and Mele, 2015). However, social contagion doesn't always hold, with one study suggesting that whilst receiving help may lead to the recipient engaging in more helping behaviour, observing someone else helping may lead to the observer engaging in less generous behaviour (Tsvetkova and Macy, 2014). In general, studies demonstrating real life person to person effects of social contagion are rare. In this study I asked if cyclists change their light jumping behaviour in response to the action of other cyclists. I predict that cyclists will copy both jumping and stopping behaviour.

Otherwise, research on cyclists has tended to focus on cyclist safety and accident risk. The major hypothesis in this field is "Safety in Numbers" which suggests that both pedestrians and cyclists are safer when there are more of them. (Hamilton, 1971; Jacobsen, 2015). Whilst there is good empirical support for this hypothesis, there is very little work to identify the proximate mechanisms that mediate it. Theoretical modelling based approaches have suggested that theoretically drivers avoid cyclists more when there are more of them (Thompson, Savino and Stevenson, 2016). Game theory has also been used to model road user interactions (Elvik, 2014).

In contrast to Chapter 4, here I directly investigate actions as they occur, rather than indirect evidence of previous actions. Moreover, I examine people's immediate reactions to the behaviour of others as it happens. In particular, I ask whether red-light jumping by cyclists at pedestrian crossings is influenced by the the presence and number of observers, and the behaviour of other cyclists.

5.2 Methods

5.2.1 Site selection

Pilot observations between 09/08/2016 and 30/08/2016 identified 10 appropriate light-controlled pedestrian crossings within the City of Edinburgh (Figure 5.1, Appendix 9). I chose pedestrian crossings because there is no risk from crossing car traffic, so the physical risk associated with cheating is low. I ruled out crossings with faulty lights or prohibitively low traffic volume, so that all sites experienced a minimum of 2 cyclists per hour (mean 13.08 ±1.19 SE). At each site I identified an observational zone around the pedestrian crossing, extending 10m in either direction from the control light, and incorporating the road and pavement (Figure 5.2).

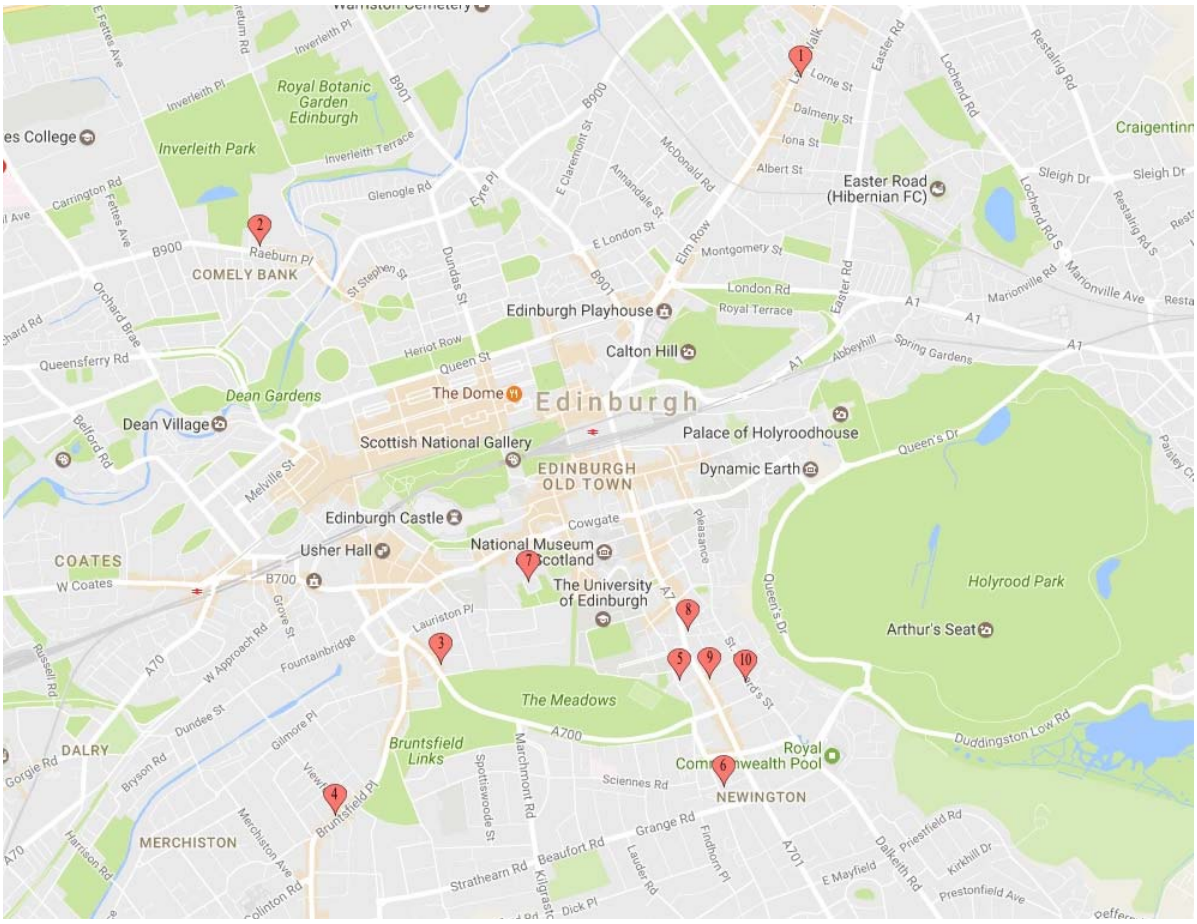

Figure 5.1 Map of experimental sites in Edinburgh

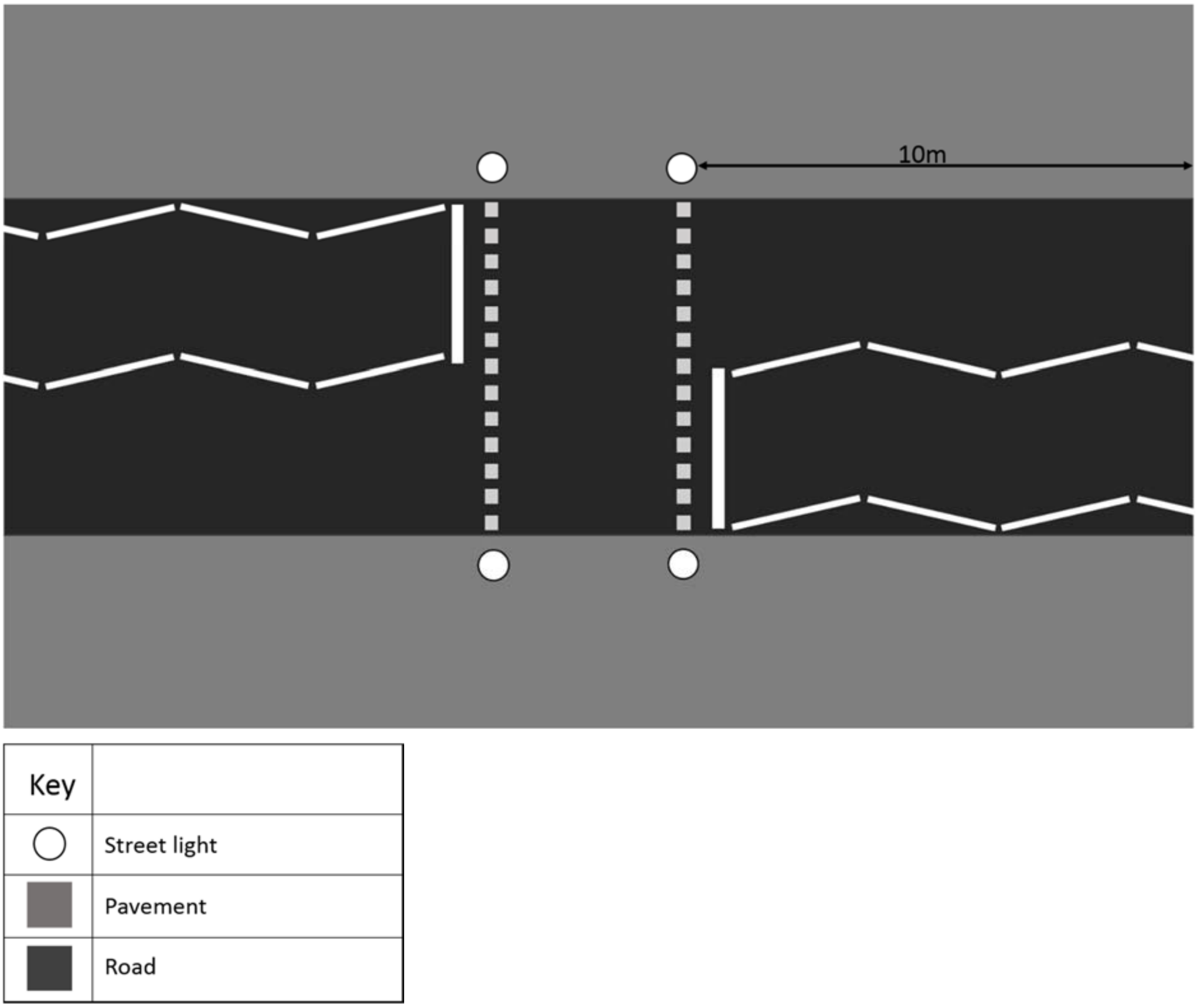

Figure 5.2: Diagram of observational zone around street lights

5.2.2 Data collection: measuring red light jumping

For both the pilot data (13 observation sessions at 8 sites) and the main investigation (44 observation sessions at 10 sites), I coded the behaviour of focal cyclists for analysis in a binomial model (1 = jump, 0 = wait). A ‘focal’ cyclist was designated as the first cyclist to arrive after a pedestrian had pressed the crossing button and the light had turned red. This means that total n for each 90 minute observation session was the number of button pushes when there was at least one cyclist present at the crossing.

As potential explanatory social variables, I recorded: (i) the number of pedestrians in the zone around the crossing (Figure 5.2), including those crossing the road; (ii) the number of pedestrians actually crossing the road; (iii) the number of vehicles in the zone around the crossing (Figure 5.2); (iv) the total number of cyclists accumulating at the crossing during that red light phase; and (v) the number of those cyclists that jumped the red light. I also recorded environmental variables: (i) time; (ii) rain (none, light, heavy); and (iii) wind (none, light, strong).

5.2.3 Pilot study: Simulation and Power Analysis

In order to estimate appropriate sample sizes for the main investigation, I conducted a power analysis using the pilot data (13 observation sessions at 8 sites).

I constructed a binomial generalised linear mixed model (GLMM), with behaviour of the focal cyclist (1=jump, 0=wait), as the response variable. Number of pedestrians and number of vehicles were fitted as fixed effects. Observation site and session were fitted as random effects (Equation 5.1).

Equation 5.1 Model for predicting cyclists jumping behaviour based on number of pedestrians and vehicles present

Jump ~ pedestrians + vehicles + (1|site) + (1|session)

I used the effect sizes generated by this model to simulate new response variables. For the fixed terms I generated dummy data by sampling randomly from the pilot data. For the random effects I sampled from a normal distribution with mean and standard deviation taken from the initial model. I used Equation 5.2 to generate response terms on the log odds scale. I then carried out an inverse logit operation to convert these to probabilities and sampled from the binomial distribution to give a response term in 0's and 1's.

Equation 5.2 Generating simulated response variables based on dummy data

y = intercept + slope(pedestrians).people + slope(vehicles).cars + Rsite + Rsess

I ran the original model on the simulated data, saving the output coefficients and p values. I iterated this process 10,000 times for four different numbers of "sessions" and four different numbers of "observations per session". For each combination of sessions and number of observations per session I calculated the power for both observations on both pedestrians and vehicles by dividing the number of significant results by the total number of runs (Table 5.1, Table 5.2).

This analysis indicated that ≥700 observations, partitioned into number of sessions and number of observations per session, would be sufficient to detect real effects. Therefore I planned my main investigation to include at least 800 observations (final total = 863 observations across 44 sessions, with an average of 19.6 observations per session).

Table 5.1 Simulation results for Pedestrians

Number of sessions:	**50**	**70**	**90**	**100**
Number of observations /session:				
5	0.3559	0.5170	0.6499	0.7038
10	0.7086	0.8590	0.9360	0.9601
15	0.8923	0.9863	0.9930	0.9959

Table 5.2 Simulation results for Vehicles

Number of sessions:	**50**	**70**	**90**	**100**
Number of observations /session:				
5	0.4859	0.6132	0.7301	0.7731
10	0.7743	0.8948	0.9529	0.9700
15	0.9162	0.9761	0.9959	0.9973

5.2.4 Main investigation

Between 01/09/2016 and 14/10/2016 we conducted 44 observations of 90 minutes at 10 sites, during morning (08:00-09:30) and afternoon (16:30-18:00) rush hours, with a minimum of two morning and two afternoon sessions at each site (average = 2.2, max = 4).

5.2.4.1 The effect of observers on red light jumping

I analysed the full data set using a binomial GLMM with behaviour of the focal cyclist as the response variable (1=jump, 0=wait), and site, session and observer as random effects. I fitted number of pedestrians and number of vehicles as the fixed effects of interest, conducting a partial f test of each term by dropping it from the model and comparing the reduced model to the full model.

5.2.4.2 The effect of other cyclists' behaviour on red light jumping

I restricted the data to instances where at least two cyclists were present (216 observations across 39 sessions at 10 sites) and constructed a binomial GLMM. Here, I used the behaviour of the focal cyclist a fixed effect (i.e. as a treatment factor, with two levels Jump or Wait), and analysed the behaviour of all other cyclists as the response variable (1=at least one other cyclist jumped, 0 = all other cyclists waited). I fitted number of cyclists, number of pedestrians and number of vehicles as additional fixed effects. Site, session and observer were fitted as random effects. I conducted partial f test on each term by dropping it from the model and comparing the reduced model to the full model.

5.2.5 Experimental Methods

I experimentally tested the effect of one cyclist's behaviour on others using a stooge cyclist who stopped at red lights in front of another cyclist. I ran 49 successful trials.

A major challenge with this experiment was ensuring that the stooge reached the light at the right time, made more difficult because lights varied in the latency between button push and turning red (Figure 5.3). To address this, I recorded how long it took each light to change. For each set of light, I took the modal changing time and measured how far the stooge cyclist could cycle in the modal time. The stooge cyclist then positioned themselves this distance from the traffic lights.

When the stooge saw another cyclist approaching, they began to cycle, and a research assistant pushed the button at the light. The stooge then stopped at the light, such that the approaching cyclist reached the crossing immediately after the stooge.

The research assistant then recorded the standard variables, before crossing themselves. Coordinating these elements was difficult: approaching cyclists could travel faster than the stooge; light changing time was unpredictable, and pedestrians pressed the button at inconvenient moments.

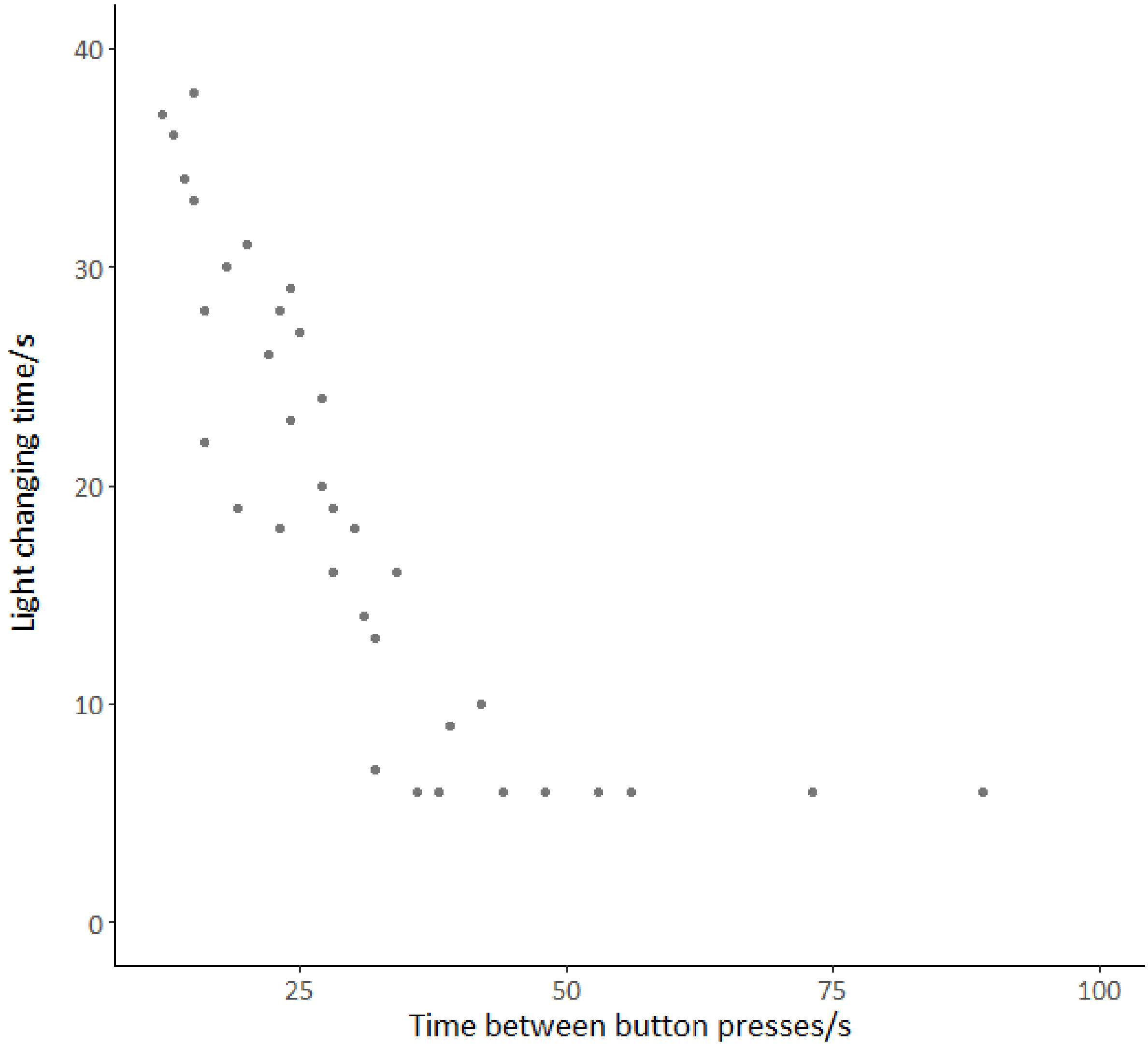

Figure 5.3 Example of time to light changing at the Rankeillor Street site

To analyse the experiment, I set the experimental data as Treatment and compared it against the observational data as Control, analysed the data with a binomial GLMM. For the Treatment data, I used the behaviour of the cyclist arriving behind the stooge as the response variable (1 = Jump, 0= Wait). For the Control data, I used the behaviour of the original focal cyclist as the response variable (1 = Jump, 0= Wait). Treatment (Control or Treatment), number of pedestrians and number of vehicles were fitted as fixed effects, and session, site and observer as random effects. I tested if treatment had a significant effect by dropping it and carrying out a partial f test with the reduced model.

Note that the obvious reciprocal of this experiment (a stooge cyclist deliberately jumping the light in front of another) was impossible since it entailed actively breaking the law.

5.2.6 Analysis

All analyses were carried out in the lme4 package of R version 3.3.1, RStudio 1.0.136.

5.2.7 Ethics Statement

All observational and experimental protocols were reviewed and approved by the School of Biological Sciences ethical review committee.

5.3 Results

5.3.1 The effect of observers on red light jumping

The probability of a focal cyclist jumping the lights decreased as the number of pedestrians increased (Figure 5.4, Table 5.3). When there were no pedestrians present, more than 29% of cyclists jump the lights.

The number of vehicles had no effect on cyclists jumping behaviour, (Table 5.3).

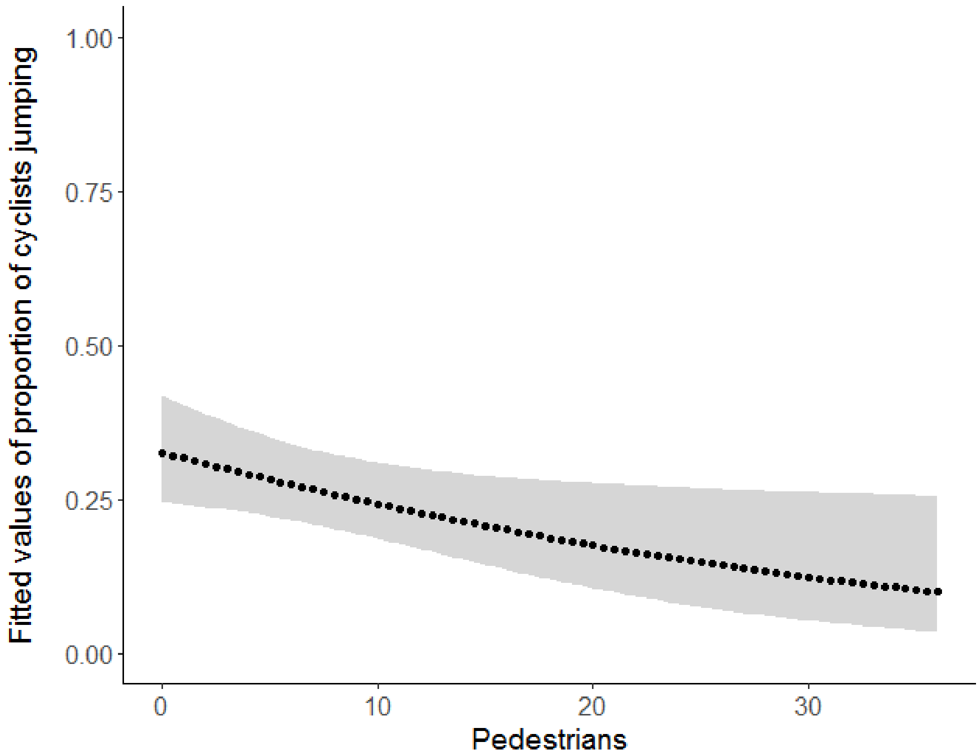

Figure 5.4 The effect of number of pedestrians at a crossing on the probability that a focal cyclist jumps a red light. The model predictions for this plot used data which marginalised sources of error from all fixed and random effects aside from pedestrians. The grey area represents a 95% confidence interval.

Table 5.3 GLMM investigating variables affecting the probability that a focal cyclist jumps a red light (n=863 from 44 observation sessions at 10 sites)

Fixed Effects	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	0.544 (0.325, 0.901)	- 2.377	0.0175
Pedestrians	0.960 (0.924, 0.996)	- 2.143	0.0321
Vehicles	0.940 (0.812, 1.089)	- 0.826	0.4089
Random Effects	**Variance**		
Session	0		
Site	$1.75 \times 10^{(-1)}$		
Observer	$5.65 \times 10^{(-10)}$		

5.3.2 The effect of other cyclists’ behaviour on red light jumping

When the focal cyclist jumped the lights, the following cyclists were more likely to jump the lights (Figure 5.4, Table 5.4), with at least one other cyclist jumping 24% of the time. When the focal cyclist stopped, at least one other cyclist only jumped 6% of the time. The total number of cyclists had no effect (Table 5.4).

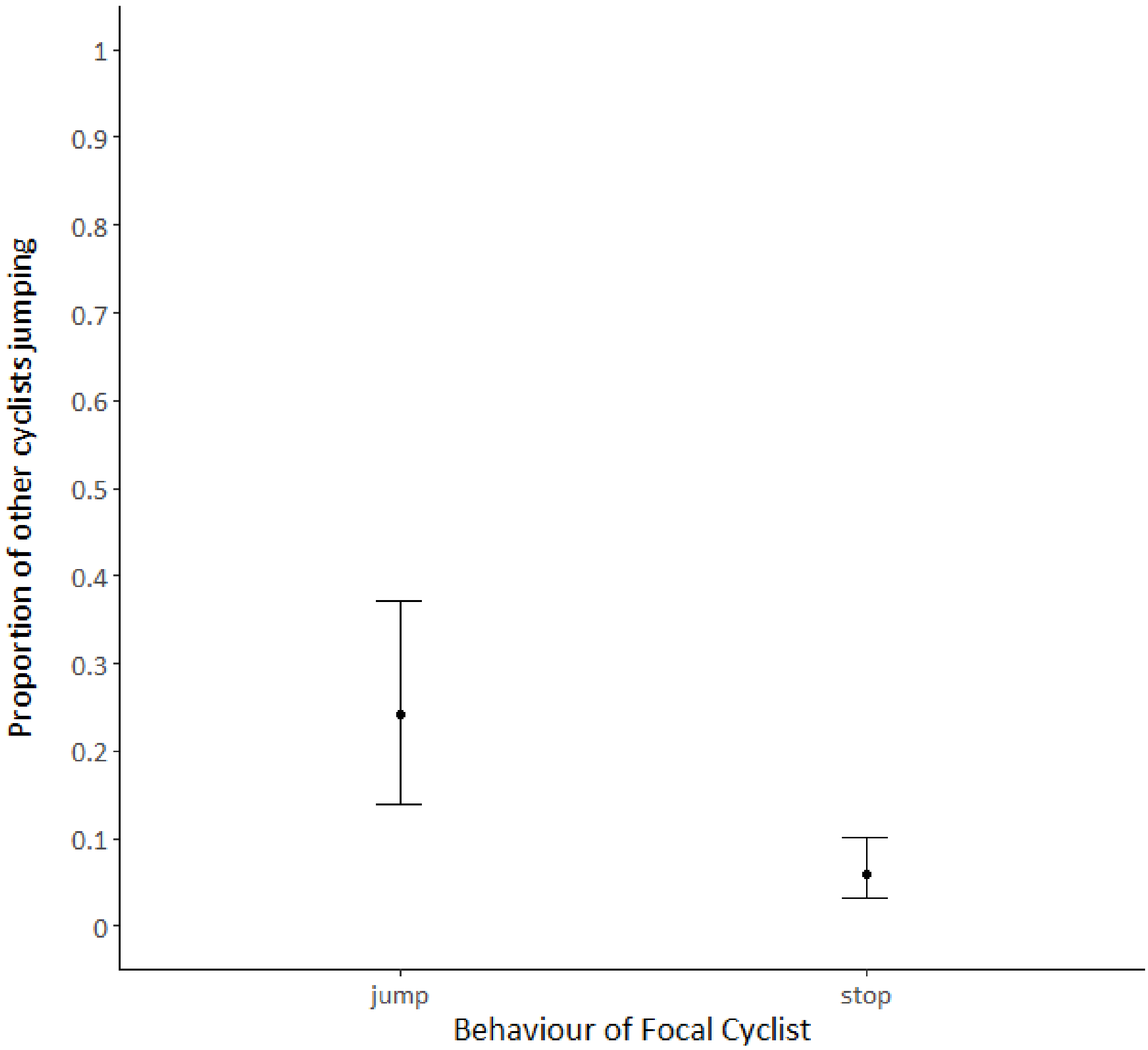

Figure 5.5 The effect of the behaviour of a focal cyclist on the probability that at least one other cyclist jumps a red light, jump n=58, stop n=158, (model estimate ±95% binomial confidence interval).

Table 5.4 GLMM investigating the effect of the behaviour of a focal cyclist on the probability that at least one other cyclist jumps a red light (n jump = 58, n stop = 216)

Fixed Effects	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	0.234 (0.025, 2.183)	-1.296	0.1950
Treatment	3.215 (1.321, 7.982)	2.597	0.0094
Pedestrians	0.986 (0.889, 1.083)	-0.279	0.7800
Vehicles	0.830 (0.551, 1.250)	-0.909	0.3631
Total Cyclists	0.859 (0.359, 1.718)	-0.391	0.6959
Random Effects	**Variance**		
Session	0.0148		
Site	0.2545		
Observer	0		

5.3.3 Stopping Experiment

Stopping by a stooge had no detectable effect on the probability of a focal cyclist jumping a light (Table 5.5, Figure 5.6).

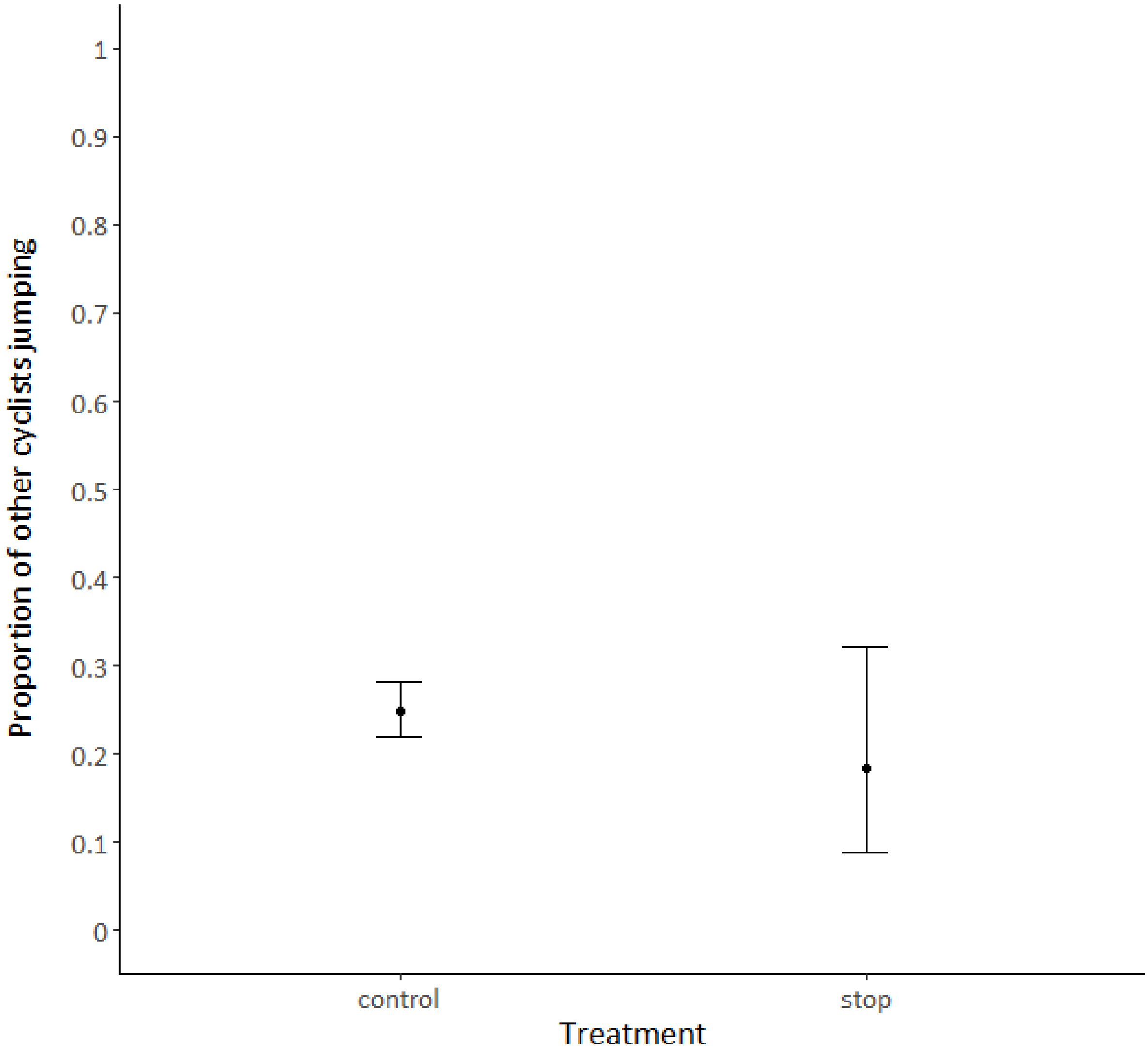

Figure 5.6 The effect of stopping by a stooge on the probability that a focal cyclist jumps a red light, control n = 863, stop n = 49 (model estimates ± 95% confience interval).

Table 5.5 GLMM investigating the effect of stopping by a stooge on the probability that a focal cyclist jumps a red light (n control = 772, n stop =49)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
(Intercept)	0.555 (0.335, 0.908)	-2.362	0.0182
Treatment	0.664 (0.291, 1.376)	-1.046	0.2958
Pedestrians	0.963 (0.928, 0.998)	-2.017	0.0437
Vehicles	0.921 (0.801, 1.060)	-1.147	0.2514
Random Effect	**Variance**		
Session	0		
Site	1.625×10^{-1}		
Observer	1.583×10^{-1}		

5.4 Discussion

In this chapter I made two central predictions: (i) That cyclists would jump red lights less when their behaviour was more observable and; (ii) That cyclists will copy others jumping and stopping behaviour. The first of these predictions is well supported, for each additional pedestrian the proportion of cyclists jumping declines by 4% (Figure 5.4, Table 5.3). There are several possible explanations for this. Although the experimental situation is anonymous, cyclists may still care about their external image score. This may not be a rational behavioural adjustment but it may be the case that this behaviour is not actively harmful and that individuals who behave in this way gain more advantage from other reciprocal interactions elsewhere in their lives (Nowak and Sigmund, 1998). In order to study this effectively it would be necessary to acquire data regarding the cooperative behaviour of individuals in this study in other settings.

A further explanation could be that individual cyclists felt that when there were more pedestrians there was a higher risk of collision. The measure of pedestrians included the number of pedestrians crossing the road whilst the light was red. It could be the case that there were either a higher number of pedestrians in the road leading to cyclists being less likely to jump the light to avoid a collision or a higher number of pedestrians on the pavement close to the crossing who might decide to cross the road at any moment, which might also lead to a collision. In order to disentangle the effects of actual risk of collision and social risk of being seen jumping an alternative study might need to be done, where, for example, variation in light jumping behaviour is studied with no pedestrians crossing. Or variation in light jumping behaviour where the number of pedestrians is held constant.

Whilst cyclists seem to change their behaviour with the number of pedestrians present they are unaffected by the number of vehicles. The power analysis that I carried out prior to beginning formal observational work suggests that I should be able to detect this effect reliably (Table 5.1, Table 5.2). There could be several explanations for this result. Several sociologists have suggested that in the urban landscape pedestrians and cyclists do not view motor vehicles as people but instead see them as objects (Taylor, 2003). This could explain why cyclists perhaps do not view cars as a social risk to their reputations, and thus do not respond to them in this way. However, motor vehicles also pose a higher actual risk of dangerous collision to cyclists than pedestrians. It is therefore interesting that cyclists do not adjust their behaviour according to a change in actual risk. Possible explanations for this could include that this study took place at pedestrian crossings. At such crossings the cost of breaking traffic laws for motorists is high if observed by a police officer or caught on CCTV, this means that motorists may be more likely to behave in a predictably less risky way. Not only is the punishment for motorists higher than for cyclists breaking traffic rules but the risk of causing significant injury to cyclists and pedestrians is substantially higher for motorists. Again meaning that motorists are more likely to behave in a predictable way at pedestrian crossings. This may mean that cyclists do not pay a great deal of attention to them. It would be interesting to investigate the role of attention in how cyclists go about making cooperative decisions in this context. Finally, cyclists apparent lack of regard for changes in actual risk provides some support for the concept that it is changes in social risk that is driving their behaviour change with varying numbers of pedestrians.

When individuals are exposed to circumstantial evidence, like faeces, this tells them little about the actual proportion of individuals cheating, or even the number of people playing the game. However, when directly observing behaviour an actor has a clear idea of the proportion of cheats.

For my second hypothesis that cyclists will copy both the jumping and stopping behaviour of preceding cyclists my results are less clear cut. I addressed the question in two ways. Firstly, I carried out a natural experiment by restricting my dataset to instances where the focal cyclist either jumped or stopped and was followed by a number of other cyclists. In this experiment both jumping and stopping appeared to have a significant effect on the behaviour of following cyclists. Secondly, I carried out an experiment where a stooge cyclist stopped at a red light and I observed the behaviour of following cyclists. I was unable to carry out a jumping experiment as this is illegal and would breach ethical guidelines. In this second experiment the stooge had no effect and experimental subjects jumped at the same rate as controls

Explaining these two, somewhat conflicting, results is complex. The first result implies that there is some type of social contagion occurring and cyclists are jumping more when they see others jump and stopping more when they see others stop. However, there could be several other explanations for this behaviour. For example, there could be a bystander effect where all following cyclists are effectively bystanders and simply copy whatever the first cyclist has done as being the local norm. There is a subtle difference between these two explanations. In social contagion an individual will emulate another's behaviour, how frequently this happens can change linearly with the number of individuals. In the bystander effect, there is a threshold where new individuals arriving may detect a social norm and the change in behaviour may not change linearly with the number of individuals performing the behaviour. The "safety in numbers" hypothesis could also apply with cyclists feeling safer in undertaking a more risky behaviour like light jumping when others are already doing it (Jacobsen, 2015). An alternative explanation, which could account for the second result, is that cyclists are all making independent decisions based on similar cues. So when these cues are conducive to light jumping behaviour more cyclists are likely to jump. For example, the only significant explanatory variable in the model I used to analyse the stopping experiment was pedestrian density (Table 5.5). A final factor that may be important when considering these results is that we may be missing some social effects as a result of physical barriers. For example, although in my stopping experiment having a stopped stooge cyclist at the lights did not lead to a reduction in jumping behaviour, it could be the case in my naturalistic experiment that absence of stopped cyclist may have led to more jumping behaviour in the naturalistic experiment.

In this chapter I made use of a simulation to analyse how much data I needed to collect to detect real effects. This technique was enormously useful as it allowed me to establish rigorously how much data would be sufficient prior to beginning formal observation work. This meant that my data collection was both more efficient and my results more reliable. There were other parts of this study that were less strong, a formal stopping experiment would have established the causality of jumping and stopping behaviour in cyclists but this was not possible due to legal and ethical constraints.

In conclusion, cyclists show a clear preference for jumping more when there are fewer pedestrians present, however, more work would be required to establish if this behaviour is as a result of their perception of collison risk or to protect their social reputations. It is unclear whether social contagion is influencing cyclists' decisions to jump red lights, although my initial naturalistic experiment suggests that this may be the case, there could also be a number of other explanations and my formal stopping experiment did not demonstrate such contagion.

Chapter 6 - Discussion

The success of human society is arguably due to our ability to cooperate with non-relatives, despite selection to cheat (Dunbar, 2003). There are several mechanisms which have been identified which stabilise cooperation, chiefly, reciprocity and punishment (West, Griffin, & Gardner, 2007). The studies I have reported examined variation in cooperative behaviour and the extent to which cooperation is stabilised by these effects. Here, I will: (1) Briefly summarise my key findings; (2) Discuss their implications; (3) Discuss my methodology and future research directions; (4) Discuss potential applications of this work.

6.1 Key findings

6.1.1 Chapter 2

I showed unambiguously that both context and the level of pay-off affect cooperative decisions. First, people behave very differently in laboratory and natural environments given similar pay-offs, with people giving at a much lower frequency outside the laboratory. Furthermore, I showed that people's perception of their pay-off matters when they make cooperative decisions, for example, whether the money they are giving is a recent windfall or earned money. When pedestrians are given a small endowment their rate of cooperation increases, even when they are not under observation. If participants are given a small endowment whilst under direct observation in the street, their rate of cooperation becomes very high, with people giving at a higher frequency than is seen in the laboratory.

6.1.2 Chapter 3

I showed that several social effects correlate with cooperative behaviour. For example, pedestrians were far more likely to give when on their own than when in a group. Furthermore, crowd density was important: donation frequency declined as the density of pedestrians on a street increased. The type of recipient made a difference too: charitable collectors received donations at higher rates than both buskers and mendicants, with mendicants receiving the least. In this study I also examined the effect of social contagion by testing whether experimental donations to mendicants induced donation behaviour. Experimental donations did not yield an increase in donation rate by pedestrians. Additionally, I examined whether the sexes cooperated differently. I found that men gave less than women, an effect that appears to be specifically driven by them giving less to charitable collectors.

6.1.3 Chapter 4

The experiments and observations yielded very few results that differed from my null hypotheses. For example, faeces abandonment appeared to be independent of house price, percentage house ownership, number of dogs, and number of dog walkers. However, The absence of a relationship between the number of dogs and level of dog faeces does indicate that there was some type of human social game being played, the exact nature of which remains unclear. Furthermore, faecal abandonment was not affected by experimental manipulation of dog fouling, suggesting that people are not sensitive to evidence of variation in cheating by others. . When I investigated additional socio-demographic factors using a multivariate analysis with a number of further predictor variables, the only one of these that was correlated with faeces abandonment was distance of a park from a primary school.

6.1.4 Chapter 5

I directly observed immediate responses to the actions of others, as opposed to circumstantial evidence of their previous behaviour. Cyclists were less likely to jump red lights as pedestrian density increased. The density of vehicles in the same area did not have an effect. When I investigated how cyclists were influenced by other cyclists' behaviour, cyclists were likely to follow the lead cyclist's behaviour, with 24% jumping when the lead cyclist jumped and 6% jumping when the lead cyclist stopped. However, experimental stopping by a lead cyclist had no effect on the rate of jumping.

6.2 Broad Significance

6.2.1 Getting out of the lab: simple economic games in the laboratory transfer poorly to the field

Chapter 2 unambiguously demonstrates that the context and pay-offs of a decision are both important. Meanwhile, results from my remaining chapters demonstrate that social effects can influence cooperative behaviour in natural systems and that pragmatic, pay-off, based explanations may account for much variation in wild cooperative behaviour. Whilst laboratory studies often try to control these variables, if they are, in fact, the main drivers of cooperative behaviour in natural situations this makes conclusions drawn in the laboratory very difficult to generalise as we cannot have an appreciation of how these decisions might change when participants are not in a laboratory context.

6.2.2 Prosociality is over-emphasised

My findings generally support traditional economic theory (Rankin, 2011). I have not found that people behave in an unexpectedly prosocial manner, or that subtle cues of others investment in a public goods game change behaviour. Whilst my findings certainly demonstrate a level of social dependence in cooperative decision-making, they also show that people will behave in an economically rational selfish way when they do not believe that this will damage their reputation. A decision, which is, in itself, economically rational.

This work generally supports conventional evolutionary theory that cooperation is problematic and requires a mechanism such as reputation or punishment to be maintained (West et al., 2007). It does not support the idea that people are unexpectedly prosocial (Burton-Chellew & West, 2012). However, some of my results do suggest that people are socially influenced when making cooperative decisions. These results provide evidence for the idea that reputational information can stabilise cooperative behaviour through reciprocity.

6.3 Specific Implications

6.3.1 The difference between lab and field: The importance of context

Overall, my studies clearly demonstrate the importance of context in cooperative behaviour. Evolutionary theory predicts that individuals should only cooperate when they gain a net benefit by doing so (West, Pen, & Griffin, 2002). In contexts where it does not pay to cooperate, we expect lower levels of cooperation. Changing the context changes the potential pay-offs available to decision-makers (Maxwell N. Burton-Chellew and West, 2012). For example, wherever reputation can influence the decisions of future partners there is selection for maintaining a cooperative reputation (Cuesta *et al.*, 2015). In humans, where reputational effects appear to be both common and powerful (Fu et al., 2008; List, 2006; Yoeli et al., 2013), this may profoundly influence the outcome of laboratory games: individuals in labs may consciously ‘know’ that they are anonymous, but they are also aware that they are under observation. This may therefore trigger innate responses, making them more likely to cooperate (Burton-Chellew & West, 2012). In a natural setting, individuals are not necessarily aware that their behaviour is being observed, so perceived reputational damage is lower, and they may behave less cooperatively. It is perhaps unsurprising that we see lower levels of cooperation in natural settings, as in chapter 2. The effect of direct observation is clear in the experiment where volunteers played a dictator game in the street. In this experiment a very large proportion of participants chose to give.

Context can also change in other ways, for example, in chapter 3, I analyse the differences in donation frequency towards mendicants, buskers and charitable collectors. Amongst these recipients, buskers are arguably engaging in a reciprocal interaction with donors as they are providing a service that donors choose to pay for. Buskers accordingly received a higher frequency of donations than mendicants. Charitable collectors received a higher still proportion of donations. This may be because individuals perceived this as a more reliable or socially acceptable recipient, so effectively, their reputation might be better maintained by donating to a charitable collector than a mendicant (Lee and Farrell, 2003).

6.3.2 Cooperation in crowds observers and companions matter

Beyond simply demonstrating the profound differences between laboratory and natural contexts, my results show that cooperation is influenced by the presence of others. The most important effects are those of group size and density. People are far more likely to give when they are on their own rather than in a group. This effect is seen clearly in the study of giving to mendicants. Street donations to mendicants are contentious, and some people strongly disagree with it (Lee and Farrell, 2003). Individuals might therefore view choosing to give when they are in the company of others as potentially risky to their reputations. Alternatively, they might just be distracted. More interesting, is the effect of pedestrian density on behaviour. In chapter 3, people are more likely to donate to mendicants when there is a lower density of pedestrians. This may be caused by a diffusion of responsibility (Kautz *et al.*, 2016) . Diffusion of responsibility might underlie well documented phenomena like the bystander effect (Darley and Latane, 1968). This could also be an ultimate explanation for the propagation of social norms as once there is a social norm in place, no one individual bears the responsibility of adjusting their behaviour (Fehr and Fischbacher, 2004).

Similarly, in chapter 5, cyclists were less likely to jump the lights when there were more pedestrians around. Again this can be explained using a public goods argument. When there are more pedestrians around, the risk of reputational damage and the risk of hitting a pedestrian and being injured are greater, so the cyclist might choose not to jump the lights as it is no longer worth it (Bjørnskau, 2015). The fact that there appears to be some social contagion in light jumping behaviour can be similarly explained. If there is another person jumping the lights, the reputational costs of this behaviour may be reduced as they are spread over a larger number of people (Christakis and Fowler, 2010). Again, increasing the incentive to jump.

6.3.3 Social strategy is not everything: ecological payoffs matter

Despite the temptation to invoke adaptive strategic explanations for variation in cooperation, my results indicate that much variation may instead be driven by immediate convenience – local idiosyncrasies of time, space or demography that alter the immediate payoffs of selfishness. People might just decide whether to cooperate or not based on what is convenient for them at that moment. This broadly conforms with basic evolutionary and economic theory, since 'convenience' amounts to a lowering of the direct cost of cooperation. (West et al., 2007). Two examples from my data stand out:

1) In chapter 4, I found higher rates of dog fouling near primary schools. This may simply be driven by temporal constraints: families with limited time may combine dropping off children at school with exercising their dog, leading to lazy faeces collection in parks near schools.
2) In chapter 2, I recorded people donating to mendicants at a higher rate when they have just picked up some money off the street. This might simply be because they happen to have some money in their hand and do not have to go through the hassle of removing some from their pocket or purse.

6.3.4 Non-adaptive explanations

While my results show variation in cooperation that is predicted by evolutionary or economic theory, it is critical to note that there is unexplained variation. It is possible that much of this variation has no adaptive explanation, and is simply the product of error in perception or decision making by actors, and errors in assumption or measurement by investigators. For example, the role of attention in facilitating cooperation has not been widely discussed. This is particularly relevant to Chapter 2, where people must first notice a mendicant soliciting for donations in order to be able to decide whether or not to donate. This means that some who did not donate might have done had they noticed the mendicant, while the investigator is forced to assume perfect knowledge in all subjects.

This may seem like a trivial point but it highlights the crucial difference between work in the laboratory and the field. Datasets, like mine, that have been collected in the field are the only way to observe the variation caused by actors making errors that are specific to real contexts, like not paying attention to mendicants. Variation in error is likely to be context specific. For example, error in laboratory results may be driven by participants' confusion about rules or pay-offs (Burton-Chellew, El Mouden and West, 2016).

While unknown or unmeasured constraints on perception and processing by actors may introduce error into data sets like these, the constraints may themselves be adaptive. Simply put, selection may have influenced both which cues actors attend to, and the intensity of their responses. This may explain the positive effect on cooperation that experiments like the "seeing-eye" posters produce (Nettle, Nott and Bateson, 2012). Understanding the adaptive origins of sensory and cognitive constraints, and the extent to which they drive variation in cooperation will be a very fruitful line of future enquiry, and may help to resolve cooperation dilemmas in applied settings (Dunbar, 2003). For example, if the average person can only recall reputational information for up to 150 people, attempting to use reputational concerns to resolve cooperative dilemmas which involve more than 150 people may be a fruitless activity.

6.4 Methodological comments

6.4.1 Lessons learned and the implications for future empirical work

Designing and conducting these studies revealed a number of important considerations for future attempts to carry out ecologically valid assessments of human behaviour:

6.4.1.1 Pilot data

Collecting pilot data and optimising experimental procedures is essential. All experiments were carried out in a field environment where there is likely to be much more unexplained variation that cannot be controlled (Levitt & List, 2007).

6.4.1.2 Replication

It is important to consider how much data is sufficient to answer an empirical question. Replication allowed me to sample variation in behaviour both within treatments and across contexts. Replicating my studies within treatments allowed me to analyse the average behaviour of a group rather than the behaviour of specific individuals. Statistically, this is a more powerful approach, and allows insight into variation in cooperative behaviour. Meanwhile replication of studies across contexts allowed me to establish the importance of the particular pay-offs associated with cooperative behaviour in these contexts. For example, replicating the dictator game in a natural context seriously calls into question the generalisability of laboratory findings. Meanwhile, carrying out multiple studies of public goods game, but recording different types of variable allowed me to draw conclusions about what caused different results in these two different contexts.

6.4.1.3 Discreet observation

I made extensive use of discreet observation of human behaviour. Whilst, at times, challenging, this approach was a highly powerful one. It allowed me to gather large quantitative datasets pertaining to my research questions but more importantly allowed me to collect data on individuals who were unaware that they were being observed. This in turn, has allowed me to both critique the status quo but also allowed me to rigorously investigate the role that reputation plays in human cooperative decision-making.

6.4.2 Future Directions

6.4.2.1 Cross-cultural studies.

Finding behavioural paradigms that translate across cultures and establishing whether there are patterns that hold across all people. If there are consistent patterns this may be indicative of some absolute constraint in our evolution or cognitive abilities. If there is cross-cultural variation this may be indicative of the presence of different drivers for cooperation in different places (Dunbar, 1993; Rand, Greene and Nowak, 2012). Understanding what causes this variation between cultures might help us better understand what drives cooperation in the first instance. It would also be fascinating to understand the effect that greater global connectivity has on cooperation both at a local and international level. The implications of this type of research could be far reaching for both social policy and international law. It may be that because of particular local norms a legal framework that would work well in one place may be inappropriate in another. Alternatively, international law could be designed to accommodate different cooperative norms.

6.4.2.2 Large scale, longitudinal quantitative datasets based on standardised protocols.

My experimental design emphasised generating datasets large enough to answer my questions, but if funding were available to gain insight at a truly population level the results might be remarkable. The additional insights that can be drawn on animal populations using such long term datasets are substantial (Clutton-Brock & Sheldon, 2010). The types of questions that could be asked at this scale could include: (1) If and how cooperation has changed over time and what has driven these changes. (2) Why trends in cooperative behaviour like recycling have changed? Do Local Authority initiatives work? (3) Could we apply these lessons to other scenarios? (4) Does crowd density have an effect on behaviour? Could this be important for safety at sports matches, concerts and airports? (5) Does people's individual behaviour change with age? There is also the possibility of using data from online interactions. This type of data could be easy to collect, especially within social media organisations (Wilson, Gosling, & Graham, 2012). This approach may be an extremely fruitful line of inquiry, however, I think it would be fascinating to establish how individuals' online behaviour matches up to their real life behaviour (Dunbar, 2016). Furthermore, it would be very interesting to know how people feel consciously about the pay-offs of their online behaviour and if this influences their online and real life decision-making and if indeed these things may be inextricably linked.

6.4.2.3 Quantitative studies that identify the instantaneous drivers of decision-making

My results that demonstrate that decision-making can be density dependent in chapters 3 and 5 suggests that transient social effects can shift behaviour significantly. The difference in results between chapters 4 and 5 might also lend support to this idea. In these chapters I studied two different public goods games with the key difference being that in chapter 4 I looked at how the evidence of others decisions influenced cooperation summed over a lengthy time period, whilst in chapter 5 I observed the conditions surrounding the focal decision-makers as they made their decisions. In chapter 4 I did not detect any social effects whilst in chapter 5 I detected several. This might be evidence that the social context at the moment of decision-making is key.

6.4.2.4The role that attention might play, or more simply how effectively individuals are picking up information.

This could affect their cooperative decisions in a non-linear way. For example, having a small amount more information may change how much people cooperate, whilst having a large amount of information may not change this cooperation additionally. I think it would be fascinating to see studies that attempt to address these kinds of issues, again in a naturalistic context, and quantify the cooperative outcomes. I think it would be interesting to establish what drives what people pay attention to. It would be key to identify contexts where the experimenter can manipulate fines or reputational concerns and establish how this adjusts attention. Littering would be an obvious context where this could be studied. However, other examples could include how the roll out of charges for plastic bags has affected how much attention individuals pay to carrying extra bags.

6.4.3 Applications

At a local level, this research has a high potential to be applied. For example, having an understanding of what drives dog fouling could be key in reducing it. My study suggested that none of my hypothesised variables correlated with dog fouling, apart from possibly proximity to primary schools. This might allow local councils to, for example, target areas near primary schools for increased monitoring or fining of dog fouling behaviour. Similarly, my work on cyclists suggests an alternative policy might handle cyclist/pedestrian interactions at crossings better. Cyclists are clearly jumping at a relatively high rate, given the illegality of this behaviour. If there are insufficient funds to dramatically increase policing of this behaviour an alternative approach might be to change the legal framework surrounding crossings and to categorise cyclists separately to vehicles. It could be made legal for cyclists to cross at the lights under certain frameworks.

Finally, setting this in a global context. Human cooperation and specifically public goods games underlie many of the world's largest problems (Milinski *et al.*, 2006). This thesis suggests that the key determinants of cooperative behaviour are social context and the actual pay-offs that individuals receive. This is enormously powerful knowledge when considering how to persuade people to change their behaviour. For example, by encouraging people to reduce their greenhouse gas emissions and thus counter the effects of climate change. If you know that the main ways to change behaviour are through pay-offs and reputational gain, you can explicitly reward people for using lower carbon technologies, by providing subsidies for purchasing electrical cars or purchasing their electricity from sustainable providers. The flip side of this would be to punish unsustainable behaviour, as initiatives like the congestion charging effectively do. Reputational benefits could be enhanced by essentially making environmentally friendly behaviour "cool" such that individuals not engaging in it receive fewer reputational benefits.

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Appendix 1

I confirm that I give my consent to take part in this survey. I know that I can withdraw at any time, and I know how to get in touch with the researchers if I have any further questions or want a copy of the results.

What is your email address?

You are being given £5, you can keep as much as you like which will be emailed to you in the form of an Amazon voucher after this survey, whatever is left will be given to a homelessness charity. How much would you like to keep?

£0

£1

£2

£3

£4

£5

How often do you give money to homeless people?

Never

Daily

Weekly

Monthly

Yearly

Why do you give money to homeless people?

Would you be more likely to give money to a homeless person if you saw someone else giving money?

Yes

No

Not Sure

I don't give money to homeless people

Are you more likely to give money when you are on your own or with other people?

On my own

With other people

Not Sure

I don't give money to homeless people

Which of the following are most likely to affect whether or not you donate?

Dog

Sign

Cup or hat

Cup or hat already containing money

I don't give money to homeless people

Other (please specify)

Thank you for completing this Survey

Appendix 2

THE UNIVERSITY *of* EDINBURGH
School of Geosciences

Verbal Consent Script (interviews) – English version

I am a student at the University of Edinburgh. I'm studying social behaviour and am interested in why people give money to people on the street.

If you agree to participate in this study, I will ask you questions about how often you give money to homeless people and buskers, and why you do so. This usually lasts between 5 and 10 minutes. Some of these may be personal or sensitive questions. You are free to choose not to answer any question. I will also stop taking notes if you prefer. You are also free to end the conversation at any time. If you decide to stop the conversation, you do not have to give me a reason why.

Anything you tell me will be private. I will not use your name or any details that might identify you when I write and publish my work. I will not tell anyone that we have had a conversation, and I will not reveal any personal details. My notes will be stored securely.

You will not be paid for taking part in this study. Helping me with this project will not affect your rights in any way.

Do you have any questions about me, my project, or this conversation before we begin?

My supervisor is Matt Bell. If you have any concerns about this study or my behaviour, you can contact him. If you need help sending him an email, I will help you.

You can contact me at a.l.munro-faure@sms.ed.ac.uk, or +44 7917734648 while I am in the UK.

[give paper with contact information on to participant]

Contact Information

This research has been reviewed and approved by the Edinburgh University Research Ethics Board. If you have any further questions or concerns about this study, please contact:

Amy Munro-Faure

Appendix 3

Mendicant Data Configuration

Observations were made in two formats: (i) with a pen and paper and (ii) automated on an iPad. The initial pilot observations and the first observations in the final dataset were made using datasheets and a pen and paper. I optimised this process by changing to using an iPad and the Animal Behaviour Pro application to collect data (Newton-Fisher, 2012). This removed the necessity for a data entry step, which was advantageous both in terms of efficiency and avoidance of errors. The same underlying data were collected in both protocols. The two observational data collection protocols used meant that the final data required substantial configuration prior to analysis. The iPad data was also advantageous in that each observation had an absolute time stamp which enabled me to analyse social contagion and density effects.

```
### Homelessness Data Configuration 09_03_2017

library(plyr)
library(dplyr)
library(stringr)
library(reshape2)
library(data.table)

options(stringsAsFactors = F)

setwd("M:/Homelessness/Observation Data/Ipad
Data/databasefiles_donttouch_27_04_17")

###import all data and merge into one csv
#mydata<-do.call(rbind, lapply(list.files(path="pooled_sorted_ipad_data", pattern=".csv",
full.names=T), read.csv, h=T))
#mydata$Log_File_Name<-str_trim(mydata$Log_File_Name, side="left")
#mydata$Notes<-str_trim(mydata$Notes, side="both")
#mydata$Event_Type<-as.factor(mydata$Event_Type)
#mydata<-mydata[!(mydata$Event_Type == " State stop"),]
#write.csv(mydata, file=mergeddata.csv)
#The merged datafile was then processed manually checking for notes and pauses. How
this was done is recorded in
#the word document "Steps in Excel merggedata", the excel file "merged data notes log
(Autosaved)". If this process
#is repeated these could now be written into code relatively easy but for time purposes I
won't do this now 27_04_2017.

#Pause_obs adjustments that needed to be made after calculating "time since last donation
are also incorporated in the above dataset"

mydata<-read.csv("mergeddata_prep_for_reentry_final_check.csv", h=T)

###reference data to append
refdata<-read.csv("Reference worksheet_final.csv", h=T)
```

```
combdata<-left_join(mydata, refdata, by="Log_File_Name", copy=FALSE)

##checking obs_nos
#write.csv(combdata, file="obscheck.csv")

###Remove useless and replicated columns
combdata$Modifier_3<-NULL
combdata$Modifier_4<-NULL
combdata$Modifier_5<-NULL
combdata$Modifier_6<-NULL
combdata$Other_Modifiers<-NULL
combdata$Observer_ID<-NULL
combdata$date<-NULL
combdata$Receiver<-NULL
combdata$Observation_Name<-NULL
combdata$Duration_s<-NULL
combdata$location<-NULL
combdata$notes<-NULL
combdata$Event_Type<-NULL
combdata$obs.no<-NULL

### Turn actor into male and female columns
Actor<-str_trim((as.character(combdata$Actor)), side="both")

####Females
onef<-ifelse(Actor=="1f", 1, 0)
twof<-ifelse(Actor=="2f", 2, 0)
threef<-ifelse(Actor=="3f", 3,0)
fourf<-ifelse(Actor=="4f", 4,0)
fivef<-ifelse(Actor=="5f", 5,0)
sixf<-ifelse(Actor=="6f", 6,0)
onefonem<-ifelse(Actor=="2x", 1, 0)
oneftwom<-ifelse(Actor=="F+2m", 1, 0)
onefthreem<-ifelse(Actor=="F+3m", 1, 0)
oneffourm<-ifelse(Actor=="F+4m", 1, 0)
oneffivem<-ifelse(Actor=="F+5m", 1,0)
twofonem<-ifelse(Actor=="2f+m", 2,0)
twoftwom<-ifelse(Actor=="2f+2m", 2,0)
twofthreem<-ifelse(Actor=="2f+3m", 2,0)
twoffourm<-ifelse(Actor=="2f+4m", 2,0)
threefonem<-ifelse(Actor=="3f+m", 3, 0)
threeftwom<-ifelse(Actor=="3f+2m", 3, 0)
threefthreem<-ifelse(Actor=="3f+3m", 3,0)
fourfonem<-ifelse(Actor=="4f+m", 4,0)
fourftwom<-ifelse(Actor=="4f+2m", 4, 0)
fivefonem<-ifelse(Actor=="5f+m", 5, 0)
```

```
Females<-(onef+twof+threef+fourf+fivef+sixf+
        onefonem+oneftwom+onefthreem+oneffourm+oneffivem+
        twofonem+twoftwom+twofthreem+twoffourm+
        threefonem+threeftwom+threefthreem+
        fourfonem+fourftwom+
        fivefonem)

###Males
monem<-ifelse(Actor=="1m", 1, 0)
mtwom<-ifelse(Actor=="2m", 2, 0)
mthreem<-ifelse(Actor=="3m", 3,0)
mfourm<-ifelse(Actor=="4m", 4,0)
mfivem<-ifelse(Actor=="5m", 5,0)
msixm<-ifelse(Actor=="6m", 6,0)
monefonem<-ifelse(Actor=="2x", 1, 0)
moneftwom<-ifelse(Actor=="F+2m", 2, 0)
monefthreem<-ifelse(Actor=="F+3m", 3, 0)
moneffourm<-ifelse(Actor=="F+4m", 4, 0)
moneffivem<-ifelse(Actor=="F+5m", 5,0)
mtwofonem<-ifelse(Actor=="2f+m", 1,0)
mtwoftwom<-ifelse(Actor=="2f+2m", 2,0)
mtwofthreem<-ifelse(Actor=="2f+3m", 3,0)
mtwoffourm<-ifelse(Actor=="2f+4m", 4,0)
mthreefonem<-ifelse(Actor=="3f+m", 1, 0)
mthreeftwom<-ifelse(Actor=="3f+2m", 2, 0)
mthreefthreem<-ifelse(Actor=="3f+3m", 3,0)
mfourfonem<-ifelse(Actor=="4f+m", 1,0)
mfourftwom<-ifelse(Actor=="4f+2m", 2, 0)
mfivefonem<-ifelse(Actor=="5f+m", 1, 0)

Males<-(monem+mtwom+mthreem+mfourm+mfivem+msixm+
        monefonem+moneftwom+monefthreem+moneffourm+moneffivem+
        mtwofonem+mtwoftwom+mtwofthreem+mtwoffourm+
        mthreefonem+mthreeftwom+mthreefthreem+
        mfourfonem+mfourftwom+
        mfivefonem)

### combining and testing
combdata<-cbind(combdata, (as.data.frame(cbind(Males, Females))))
combdata$Actor<-NULL


####Turn Donor ID data into two columns
Mod1<-(combdata$Modifier_1)
Mod1<-str_trim(Mod1, side="both")
```

```
Mod1[which(is.na(Mod1))]<-""


donor.sex<-substr(Mod1, 1, 2)
donor.age<-substr(Mod1, 3, 9)

###Turn the very limited donor 2 data into 2 columns
Mod2<-combdata$Modifier_2
Mod2<-str_trim(Mod2, side="both")
Mod2[which(is.na(Mod2))]<-""


donortwo.sex<-substr(Mod2, 1, 2)
donortwo.age<-substr(Mod2, 3,9)
Other_donor<-ifelse(donortwo.sex=="M "|donortwo.sex=="F ", 1, 0)

##Combine and remove
combdata<-cbind(combdata, donor.sex, donor.age, donortwo.sex, donortwo.age,
Other_donor)
combdata$Modifier_2<-NULL
combdata$Modifier_1<-NULL


###Code behaviour as 0's and 1's

combdata$Behavior<-ifelse(combdata$Behavior==" No donation", 0,1)

###Add additional columns from preipad data to combdata

###Add extra columns to define ipad vesus preipad

ipad<-rep("ipad", length(combdata$Date_ymd))
combdata<-cbind(combdata, ipad)

###Kids
combdata$Notes<-str_trim(combdata$Notes, side="both")

ka<-ifelse(combdata$Notes=="K", 1, 0)
kb<-ifelse(combdata$Notes=="Kk", 2, 0)
kc<-ifelse(combdata$Notes=="Kpp", 1, 0)
kd<-ifelse(combdata$Notes=="Kkp", 2, 0)
ke<-ifelse(combdata$Notes=="Kdon", 1, 0)
kf<-ifelse(combdata$Notes=="Kkpp", 2, 0)
kg<-ifelse(combdata$Notes=="K kdon", 2, 0)
kh<-ifelse(combdata$Notes=="Kkkk", 4, 0)
ki<-ifelse(combdata$Notes=="Kkkkkkkk", 8, 0)
```

```
kj<-ifelse(combdata$Notes=="Kdon p", 1, 0)
kk<-ifelse(combdata$Notes=="kdon", 1, 0)
kl<-ifelse(combdata$Notes=="Kkp kdon", 3, 0)
km<-ifelse(combdata$Notes=="Kkkp", 3, 0)
kn<-ifelse(combdata$Notes=="Kkkkp", 3, 0)
kn<-ifelse(combdata$Notes=="Kkw", 2, 0)
ko<-ifelse(combdata$Notes=="Kk kdon", 3, 0)

kidscomb<-(ka+kb+kc+kd+ke+kf+kg+kh+ki+kj+kl+km+kn+ko)
###Prams
pa<-ifelse(combdata$Notes=="Ppp", 3, 0)
pb<-ifelse(combdata$Notes=="P", 1, 0)
pc<-ifelse(combdata$Notes=="Pp", 2, 0)
pd<-ifelse(combdata$Notes=="Kpp", 2, 0)
pe<-ifelse(combdata$Notes=="Kkp", 1, 0)
pf<-ifelse(combdata$Notes=="Kp", 1, 0)
pg<-ifelse(combdata$Notes=="Kkpp", 2, 0)
ph<-ifelse(combdata$Notes=="Kdon p", 1, 0)
pi<-ifelse(combdata$Notes=="Kkp kdon", 1, 0)
pj<-ifelse(combdata$Notes=="Kkkp", 3, 0)
pk<-ifelse(combdata$Notes=="Kkkp", 3, 0)
pl<-ifelse(combdata$Notes=="Kkkkp", 4, 0)

pramscomb<-(pa+pb+pc+pd+pe+pf+pg+ph+pi+pj+pk+pl)
###Wheelchairs

wa<-ifelse(combdata$Notes=="Kkw", 1, 0)
wb<-ifelse(combdata$Notes=="Wh", 1, 0)

wheelcomb<-(wa+wb)

#Kdonations

da<-ifelse(combdata$Notes=="Kdon", 1, 0)
db<-ifelse(combdata$Notes=="K kdon", 1, 0)
dc<-ifelse(combdata$Notes=="Kdon p", 1, 0)
dd<-ifelse(combdata$Notes=="kdon", 1, 0)
de<-ifelse(combdata$Notes=="Kkp kdon", 1, 0)
df<-ifelse(combdata$Notes=="Kk kdon", 1, 0)

doncomb<-(da+db+dc+dd+de+df)

##Add in NAs for homeless ID
hom_id<-rep(NA, length(combdata$Date_ymd))

###Combine and remove
```

```
combdata<-cbind(combdata,(as.data.frame(cbind(kidscomb, pramscomb, wheelcomb,
doncomb, hom_id))))
combdata$Notes_2<-NULL
combdata$Notes<-NULL

#write.csv(combdata, file="ipadmerge.csv")

##########################
### Bring in pre-ipad data
##########################

preipad<-read.csv("pre_ipad_data_check.csv", h=T)

template<-data.frame(matrix("", nrow=22089, ncol=length(combdata)))
colnames(template)<-colnames(combdata)

###Formatting pre-ipad data to go into some columns

preipad$obs_no<-as.character(preipad$obs_no)

###Getting rid of useless columns

preipad$notes<-NULL

##start time
start<-preipad[ !duplicated(preipad$obs_no), ]
start<-as.data.frame((cbind(start$obs_no, start$Time)))
colnames(start)<-c("obs_no", "begin")
preipad<-left_join(preipad, start, by="obs_no", copy=FALSE)

##end time
end<-ddply(preipad, "obs_no", function(z) tail(z,1))
end<-as.data.frame((cbind(end$obs_no, end$Time)))
colnames(end)<-c("obs_no", "finish")
preipad<-left_join(preipad, end, by="obs_no", copy=FALSE)

##number of events
numeve<-as.data.frame(aggregate(preipad$Group.Size, by=list(preipad$obs_no),
FUN=sum))
colnames(numeve)<-c("obs_no", "number.events")
preipad<-left_join(preipad, numeve, by="obs_no", copy=FALSE)

##number of donations
numdon<-as.data.frame(aggregate(preipad$Donation, by=list(preipad$obs_no), FUN=sum))
colnames(numdon)<-c("obs_no", "number.donations")
preipad<-left_join(preipad, numdon, by="obs_no", copy=FALSE)
```

```
##Other donations

preipad$Other.Donor<-ifelse(is.na(preipad$Other.Donor)|preipad$Other.Donor==0,0,1)


### Putting pre-ipad data into columns
template$Date_ymd<-preipad$Date
template$Time_Absolute_hms<-preipad$Time
template$Time_Relative_hms<-NA
template$Time_Relative_s<-NA
template$Time_Lag_s<-NA
template$Log_File_Name<-NA

template$Behavior<-preipad$Donation
template$pause_obs<-preipad$obs_no
template$global.obs.no<-preipad$obs_no
template$start<-preipad$begin
template$end<-preipad$finish
template$loc_code<-preipad$loc_code

template$city<-"edinburgh"
template$temp<-preipad$Temperature
template$wind<-preipad$Wind
template$rain<-preipad$Rain
template$cloud<-preipad$Cloud
template$type<-"home"

template$cup<-preipad$Cup
template$dog<-preipad$Dog
template$sign<-preipad$Sign
template$money<-preipad$Money
template$age<-preipad$Age
template$sex<-preipad$Sex

template$number.events<-preipad$number.events
template$number.donations<-preipad$number.donations
template$Males<-preipad$M
template$Females<-preipad$F
template$donor.sex<-preipad$Donor.Sex
template$donor.age<-preipad$Donor.Age

template$donortwo.sex<-NA
template$donortwo.age<-NA
template$Other_donor<-preipad$Other.Donor
template$ipad<-"preipad"
```

```
template$kidscomb<-preipad$Kids
template$pramscomb<-preipad$Pram

template$wheelcomb<-as.numeric(NA)
template$doncomb<-preipad$Kdon
template$hom_id<-preipad$hom_id

### Binding together pre and post ipad data

fabdata<-rbind(template, combdata)

###checking all columns for extra spaces etc.
names(fabdata)

for (i in names(fabdata)) {
  if(class(fabdata[, i]) %in% c("factor", "character")){
    fabdata[, i] <- trimws(fabdata[, i])
  }
}

###Adding new columns

#Group Size and sex ratio
group_size<-fabdata$Males+fabdata$Females
sex_ratio<-fabdata$Females/(fabdata$Males+fabdata$Females)
fabdata<-cbind(fabdata, group_size, sex_ratio)


##checking categorical variables for misspellings
fabdata$rain[fabdata$rain=="Light"]<-"light"
fabdata$rain[fabdata$rain=="0"]<-"none"
fabdata$wind[fabdata$wind=="0"]<-"none"

fabdata<-fabdata[-22089,]

##homeless sex
fabdata$sex[fabdata$sex=="m"]<-"M"
fabdata$sex[fabdata$sex=="f"]<-"F"
fabdata$sex[fabdata$sex=="m/2f"]<-NA
fabdata$sex[fabdata$sex=="m/f"]<-NA


#write.csv(fabdata, file="fabdata.csv")

#########################
#Time since last donation
```

```
#########################
timedata<-subset(fabdata, ipad=="ipad")
timedata$Time_Absolute_hms<-str_trim(timedata$Time_Absolute_hms, side="both")
timecode<-as.numeric(as.POSIXct(timedata$Time_Absolute_hms, format="%H:%M:%S"))
timedata<-cbind(timedata, timecode)

##Liams ideas
# Turn your data frame into a 'data table'
timedt<-data.table(timedata)

# Set key
setkey(timedt, pause_obs, timecode)

# Create column that gives time of last donation, for each observation time
timedt<-timedt[Behavior == 1, .(lastdonation = timecode), key = c("pause_obs",
"timecode")][timedt, roll = TRUE]

###Trying to create a column that calculates number of donations in last two minutes

timedt<-timedt[Behavior == 1, .(last2mins = timecode), key = c("pause_obs",
"timecode")][timedt, roll = TRUE]

# Create column that calculates the difference in time back to the last donation
timedt[, time_to_lastdonation := timecode - shift(lastdonation,1,type="lag"),
by=list(pause_obs)]

##Temporal variables 08_05_17

setDT(timedata)[,pause_obs2:=.GRP,by=c("pause_obs")]

#Build reference table for donation behavior
Ref <- timedata[,list(Compare_Value=list(I(Behavior)),Compare_Time=list(I(timecode))),
by=c("pause_obs2")]

#Use mapply to get last 2 mins of donation by pause_obs
timedata[,twomin.Roll := mapply(RD = timecode,NUM=pause_obs2, function(RD, NUM) {
  d <- as.numeric(Ref$Compare_Time[[NUM]] - RD)
  sum((d <= -1 & d >= -121)*Ref$Compare_Value[[NUM]])})]

#Use mapply to get last 2 mins of donation by pause_obs
timedata[,onemin.Roll := mapply(RD = timecode,NUM=pause_obs2, function(RD, NUM) {
  d <- as.numeric(Ref$Compare_Time[[NUM]] - RD)
  sum((d <= 0 & d >= -60)*Ref$Compare_Value[[NUM]])})]

#Use mapply to get current minute of donation by pause_obs
timedata[,Roll.obs := mapply(RD = timecode,NUM=pause_obs2, function(RD, NUM) {
```

```
  d <- as.numeric(Ref$Compare_Time[[NUM]] - RD)
  sum((d <= 30 & d >= -30)*Ref$Compare_Value[[NUM]])})]

#Build reference table for group size
Ref4 <- timedata[,list(Compare_Value=list(I(group_size)),Compare_Time=list(I(timecode))),
by=c("pause_obs2")]

#Use mapply to get sum of passers by in previous 2 minutes
timedata[,beforetwomingroup.Roll := mapply(RD = timecode,NUM=pause_obs2,
function(RD, NUM) {
  d <- as.numeric(Ref4$Compare_Time[[NUM]] - RD)
  sum((d <= 0 & d >= -120)*Ref4$Compare_Value[[NUM]])})]

timedata[,beforeonemingroup.Roll := mapply(RD = timecode,NUM=pause_obs2,
function(RD, NUM) {
  d <- as.numeric(Ref4$Compare_Time[[NUM]] - RD)
  sum((d <= 0 & d >= -60)*Ref4$Compare_Value[[NUM]])})]


## pause session start time
start2<-timedata[ !duplicated(timedata$pause_obs), ]
start2<-as.data.frame((cbind(start2$pause_obs, start2$timecode)))
colnames(start2)<-c("pause_obs", "begin")
timedata$pause_obs<-as.character(timedata$pause_obs)
start2$begin<-as.numeric(start2$begin)
timedata<-left_join(timedata, start2, by="pause_obs", copy=FALSE)

##adding nas into first two minutes of rolling values

timedata$twomin.Roll[timedata$timecode-timedata$begin<=120]<-NA
timedata$onemin.Roll[timedata$timecode-timedata$begin<=60]<-NA
timedata$Roll.obs[timedata$timecode-timedata$begin<=120]<-NA
timedata$beforetwomingroup.Roll[timedata$timecode-timedata$begin<=120]<-NA
timedata$beforeonemingroup.Roll[timedata$timecode-timedata$begin<=60]<-NA

## write out csv
write.csv(timedata, file="test.csv")

plot(timedata$Roll.obs~timedata$twomin.Roll)
require(lme4)
model<-lm(Roll.obs~twomin.Roll, data=timedata)
summary(model)

head(timedata)
head(fabdata)
unique(fabdata$Date_ymd)
```

```
###Singletons only dataset
fabdata<-read.csv("fabdata.csv", h=T)

allsingle<-subset(fabdata, group_size==1)
singletonsex<-ifelse(allsingle$Males==1, "M", "F")
allsingle<-cbind(allsingle, singletonsex)
allsingle<-subset(allsingle, ipad=="ipad")

####number of passers by in previous minute
###Timecode step

allsingle$Time_Absolute_hms<-str_trim(allsingle$Time_Absolute_hms, side="both")
timecode<-as.numeric(as.POSIXct(allsingle$Time_Absolute_hms, format="%H:%M:%S"))
allsingle<-cbind(allsingle, timecode)


setDT(allsingle)[,pause_obs2:=.GRP,by=c("pause_obs")]

#Build reference table
Ref3 <- allsingle[,list(Compare_Value=list(I(group_size)),Compare_Time=list(I(timecode))),
by=c("pause_obs2")]

#Use mapply to get last 2 mins of donation by dup
allsingle[,beforetwomin.Roll := mapply(RD = timecode,NUM=pause_obs2, function(RD,
NUM) {
  d <- as.numeric(Ref3$Compare_Time[[NUM]] - RD)
  sum((d <= 0 & d >= -120)*Ref3$Compare_Value[[NUM]])})]

#adding a begin column
start3<-allsingle[ !duplicated(allsingle$pause_obs), ]
start3<-as.data.frame((cbind(allsingle$pause_obs, allsingle$timecode)))
colnames(start3)<-c("pause_obs", "begin")
allsingle$pause_obs<-as.character(allsingle$pause_obs)
start3$begin<-as.numeric(start3$begin)
allsingle<-left_join(allsingle, start3, by="pause_obs", copy=FALSE)

allsingle$beforetwomin.Roll[allsingle$timecode-allsingle$begin<=120]<-NA

head(allsingle)

write.csv(allsingle, file="allsingle.csv")
```

Appendix 4

Sex effect Models

When all the data are analysed with an interaction between the type of focal recipient and the sex of the donor, donor sex has no effect on behaviour. However, this interaction is non-significant and when dropped, a significant effect emerges with single males giving at 82% of the rate of single females. When different types of focal are analysed separately it becomes clear that this effect is almost entirely driven by a strongly significant difference in donation behaviour towards charitable collectors, with males giving at 69.9% the rate of females.

Singletons Model with Sex:Type interaction (n=26489)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
Intercept	0.052 (0.033, 0.083)	-12.544	2×10^{-16}
Pedestrian Sex: Male	0.832 (0.659, 1.047)	-1.589	0.1121
Type: Charity	1.271 (0.799, 2.000)	1.048	0.2945
Type: Mendicant	0.409 (0.282, 0.597)	-4.765	1.88×10^{-6}
Focal Sex: Male	1.000 (0.690, 1.446)	0.002	0.9986
Rain: Light	0.997 (0.693, 1.428)	-0.015	0.9877
Rain: Heavy	1.392 (0.338, 5.361)	0.480	0.6311
Density	0.978 (0.962, 0.995)	-2.552	0.0107
Pedestrian Sex: Male* Type: Charity	0.839 (0.592, 1.186)	-1.004	0.3153
Pedestrian Sex: Male* Type: Mendicant	1.154 (0.798, 0.647)	0.807	0.4196
Random Effects	**Variance**	**Standard Deviation**	
Session	0.09586	0.3096	
Location	0.13384	0.3658	
Date	0.05590	0.2364	

Singletons Model without Sex:Type interaction (n=26489)

Fixed Effect	Odds ratio (95% CI)	z Statistic	P(z)
Intercept	0.052 (0.033, 0.083)	-12.669	$< 2 \times 10^{-16}$
Pedestrian Sex: Male	0.820 (0.709, 0.947)	-2.737	0.0062
Focal Sex: Male	1.000 (0.689, 1.445)	-0.002	0.9981
Type: Charity	1.184 (0.761, 1.826)	0.773	0.4394
Type: Mendicant	0.438 (0.313, 0.616)	-4.898	9.67×10^{-7}
Rain: Light	0.998 (0.693, 1.430)	-0.011	0.9909
Rain: Heavy	1.389 (0.336, 5.731)	0.474	0.6352
Density	0.978 (0.961, 0.995)	-2.562	0.0104
Random Effects	**Variance**	**Standard Deviation**	
Session	0.09551	0.3090	
Location	0.13557	0.3682	
Date	0.05646	0.2376	

Mendicant Singletons Model (n=13427)

Fixed Effect	Odds ratio (95% CI)	z Statistic	P(z)
Intercept	0.028 (0.014, 0.056)	-9.958	$< 2 \times 10^{-16}$
Pedestrian Sex: Male	0.958 (0.736, 1.248)	-0.316	0.7524
Focal Sex: Male	0.870 (0.475, 1.593)	-0.450	0.6526
Rain: Light	0.915 (0.490, 1.707)	-0.280	0.7797
Rain: Heavy	1.215 (0.284, 5.204)	0.263	0.7926
Density	0.966 (0.937, 0.996)	-2.218	0.0266
Random Effects	**Variance**	**Standard Deviation**	
Session	0.09006	0.3001	
Date	0.13240	0.3639	
Location	0.09025	0.3004	

Busker Singletons Model (n=8270)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
Intercept	0.044 (0.023, 0.083)	-9.511	$< 2 \times 10^{-16}$
Pedestrian Sex: Male	0.830 (0.658, 1.044)	-0.602	0.109
Focal Sex: Male	1.018 (0.585, 1.770)	0.065	0.948
Rain: Light	0.670 (0.326, 1.333)	-1.152	0.249
Density	0.997 (0.969, 1.025)	-0.230	0.818
Random Effects	**Variance**	**Standard Deviation**	
Session	0.1868	0.4322	
Date	0.0285	0.1688	

Charitable Collector Singletons Model (n=4792)

Fixed Effect	**Odds ratio (95% CI)**	**z Statistic**	**P(z)**
Intercept	0.044 (0.015, 0.125)	-6.327	2.51×10^{-10}
Pedestrian Sex: Male	0.699 (0.538, 0.902)	-2.737	0.0062
Focal Sex: Male	1.759 (0.551, 5.010)	1.105	0.2693
Rain: Light	1.079 (0.730, 1.657)	0.382	0.7028
Density	0.972 (0.943, 1.003)	-1.807	0.0708
Random Effects	**Variance**	**Standard Deviation**	
Session	0.03313	0.182	
Location	0.21807	0.467	

Appendix 5

Park	Treatment
Campbell Park	Addition
Meadowspot Park	Addition
Morningside Park	Addition
King George V Park (Currie)	Addition
Malleny Park	Addition
Newcraighall Park	Addition
Clermiston Park	Addition
Redhall Park	Addition
Bingham Park	Addition
Lochend Park	Addition
Regent Road Park	Addition
Mortonhall Community Park	Control
Seven Acre Park	Control
Abercorn Park	Control
Blinkbonny Park	Control
Starbank Park	Control
Fernieside Recreation Ground	Control
Leith Links	Control
Moredun Park	Control
Calton Hill	Control
Gypsy Brae	Control
Braidburn Valley Park	Removal
Davidsons Mains Park	Removal
Easter Craiglockhart Hill	Removal
Allison Park	Removal
Joppa Quarry Park	Removal
East Pilton Park	Removal
Ratho Station Park	Removal
West Pilton Park	Removal
Hunters Hall Park	Removal
Murieston Park	Removal
Princes St Gardens	Removal

Appendix 6

Making Artificial Dog Faeces

I investigated purchasing fake faeces. These were costly and unconvincing. I was also concerned about the environmental consequences of leaving out plastic faeces for extended periods of time. Instead I manufactured fake faeces, which were more convincing, environmentally friendly and would not poisonous. I generated three prototypes and subjected them to an informal survey of 20 peers. Respondents agreed unanimously that prototype 3 was the most convincing fake dog faeces.

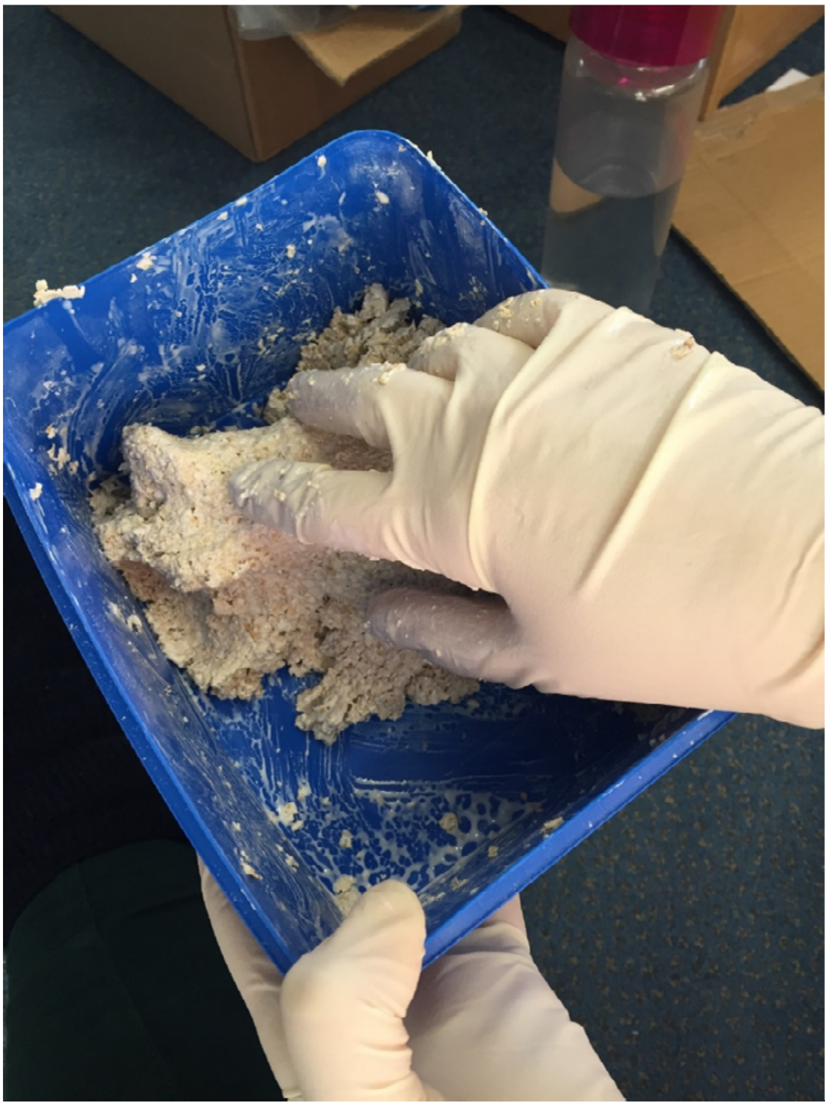
Prototype 1: This was made of plaster of paris poured into a shape that approximated soft dog faeces. It was spray painted with mushroom coloured spray paint. This prototype looked too consistent. It was advantageous in that plaster of paris is relatively inert once set and would be unlikely to be harmful to hungry dogs.

Prototype 2: This prototype was made of plumbers foam sprayed into a shape that approximated dog faeces and sprayed with mushroom coloured spray paint. This prototype had several problems: (i) they were very shiny; (ii) they were quite large and size was difficult to control; (iii) plumbers foam might be toxic to dogs (iv) they were very light and might blow away. This prototype was rejected.

Prototype 3: This prototype was made up of porridge oats and watered down PVA glue. Aliquots of the oat glue mix were rolled into sausages of varying thicknesses. The sausages were then shaped into faecal shapes. This prototype had a heterogeneous texture and I could easily vary the size of faeces to represent a variety of dog sizes. Porridge oats and PVA are not toxic to dogs.

Faeces Manufacture

All faeces was manufactured by hand. I made approximately 200 fake faecal deposits.



Appendix 7

Means and Standard Errors for Total Faeces and Total Dogs

	Mean Total Faeces	SE Total Faeces	Mean Dogs	SE Dogs
Abercorn Park	2.5	0.6454972	16.75	2.4958299
Allison Park (Kirkliston)	5.75	1.0307764	1.25	0.75
Bingham Park	3.25	1.0307764	4.25	1.652019
Blinkbonny Park	9.5	0.8660254	1	0.4082483
Braidburn Valley Park	6.25	0.75	11.5	3.6628768
Calton Hill	1	0.4082483	1.75	0.4787136
Campbell Park	2	0.4082483	1.5	0.5
Clermiston Park	1.5	0.2886751	1.5	1.1902381
Davidson's Main Park	4.25	1.9737865	11.25	1.4361407
E Pilton Park	17	4.8476799	3.75	0.6291529
Easter Craiglockhart Hill	4	0.4082483	8.5	1.9364917
Fernieside Recreation Ground	29	4.8131764	3.5	1.9364917
Gypsy Brae	30.25	3.705289	9	2.9439203
Hunters Hall Park	13	1.0801234	2	0.7071068
Joppa Quarry Park	16	2.3452079	5.25	2.3584953
King George V Park (Curry)	8	2.0412415	2.75	0.8539126
Leith Links	14.5	1.9364917	8.25	1.4930394
Lochend Park	6.5	1.7078251	14.75	1.3149778
Malleny Park	0.25	0.25	0	0
Meadowspot Park	5.75	0.75	2.25	0.9464847
Moredun Park	31.25	2.5289985	3.75	0.75
Morningside Park	21.5	3.5	4.5	2.0207259
Mortonhall Community Park	13.25	1.25	3.5	1.3228757
Murieston Park	15.5	0.6454972	3.5	0.2886751
Newcraighall Park	12.5	1.3228757	3	1.0801234
Princes St Gardens	0.75	0.25	1.5	0.9574271
Ratho Station Park	15	2.7386128	2.75	0.8539126
Redhall Park	4	1.0801234	7.5	1.5
Regent Road Park	30.75	1.1086779	2	0.4082483
Seven Acre Park	0.25	0.25	6.75	0.8539126

Starbank Park	8.75	2.3584953	1.25	0.9464847
W Pilton Park	20.75	3.1191612	4	0.9128709

Appendix 8

Our Ref: IM-FOI-2017-2571
Date: 08 December 2017

POLICE
SCOTLAND
Keeping people safe

FREEDOM OF INFORMATION (SCOTLAND) ACT 2002

I refer to your recent request for information which has been handled in accordance with the Freedom of Information (Scotland) Act 2002.

For ease of reference, your request is replicated below together with the response.

I am a current PhD Student at the University of Edinburgh. As part of my research I'm interested in the rate at which cyclists jump red lights at pedestrian crossings in Edinburgh.

I'm looking for information as to how often this behaviour is detected and punished. I was wondering if you have any records of how many individuals have been charged and/or prosecuted over the last 3-5 years for this offence?

In relation to the above I can advise you that in the last 5 years (November 2012 to November 2017) a total of 264 Conditional Offers of Fixed Penalty were issued under SGJD Code 305012 (RTA 1988 S36(1) FAIL TO COMP WITH TRAFFIC SIGN - PEDAL CYCLE).

Please note that this total will not include those detected and dealt with by way of an informal warning at the roadside or those involved in more serious offences who may have been reported direct to Procurator Fiscal for a number of other offences that may include the act of cycling through a red light, such as careless cycling etc.

In relation to the numbers provided with informal warning, I must advise that the only way to identify this would be to carry out a manual search of every Police Officers notebooks to try and extract this information. For this reason I regret to inform you that I am unable to provide you with this information as it would prove too costly to do so within the context of the fee regulations.

As you may be aware the current cost threshold is £600 and I estimate that it would cost well in excess of this amount to process your request.

As such, and in terms of Section 16(4) of the Freedom of Information (Scotland) Act 2002 where Section 12(1) of the Act (Excessive Cost of Compliance) has been applied, this represents a refusal notice for the information sought.

Should you require any further assistance concerning this matter please contact Information Management - Edinburgh on 0131 311 3901 quoting the reference number given.

If you are dissatisfied with the way in which your request has been dealt with, you are entitled in the first instance, and within 40 working days of receiving this response, to request a review of our actions and decisions.

Appendix 9

Locations of crossings for observations on cyclists

Site Location	Post Code
Leith Walk	EH6 8NY
Raeburn Place	EH4 1HH
Brougham Place	EH3 9HW
Bruntsfield Place	EH10 4ER
Buccleuch Street	EH8 9NG
Causewayside	EH9 1QG
Lauriston Place	EH3 9EQ
Nicolson Street	EH8 9EH
Rankeillor Street	EH8 9JB
St. Leonards	EH8 9QY